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COURSE OF

LECTURES ON THE BLOOD,

AND ON

THE CHANGES WHICH IT UNDERGOES DURING DISEASE,

Delivered at the College of France in 1837-8,

BY

M. MAGENDIE.

LECTURE X.

Causes of movement of the blood in the veins.—Differences between the arterial and venous circulations.—Effects of ligature of arteries on the circulation in the brain.—Progressive force of the blood in the deep and superficial veins.—Examination of the blood in a syphilitic patient.—State of the fluid in Bright's disease.—Effects of the injection of serum.

GENTLEMEN:—Notwithstanding all that has been written on the movement of the blood in the veins, there are few questions in physiology more completely enveloped in error and uncertainty. Our knowledge on the subject is limited to mere hypothesis; the most remarkable poverty in facts is everywhere apparent. The favourite idea—that which takes the lead of all others—is, that the contraction of the capillary vessels causes the progression of the fluid in the veins. Bichat maintained that this was the sole cause of impulsion; the school of Beclard, somewhat less exclusive in its dogmas, admits that the force which sets the arterial blood in motion, acts conjointly with the capillary system in carrying on the venous circulation. On the other hand, my opinion is, that the capillaries have nothing to do with the matter, and that they must be completely stripped of the pretended functions with which they have been invested. Our business on this and similar occasions, is not to divine how Nature acts, but to ascertain, by accurate experiments, the processes she adopts.

The point we are at present more immediately engaged in proving is, that the movement of the blood in the veins is chiefly effected by the heart's action, and by the elasticity of the arteries, which is itself brought into play by the contractions of the left ventricle. I shall not lose sight of the accessory powers that aid in circulating the venous blood, among which the movemepts of respiration hold the first rank. It results from the experiments made in your presence at our last meeting, that the pressure supported by the veins is very much inferior to that sustained by the arteries. Now, the principle of movement, in both systems, resides in the heart; without it the circulation in either would be at an end. Why, then, it may be asked, are not the hydrodynamic phenomena of that function identical in every point of the vascular system? Because a variety of circumstances, many of which are already known to you, and the remainder of which I shall take care to elicit experimentally in due season, prevent such from being the case. If you take a view of the general disposition of the tubes through which the blood moves, you will be forcibly struck with the difference in the mode of distribution and anastomosis that obtains in the arterial and venous systems. While the blood is propelled by the heart into tubes of decreasing dimensions, it returns towards that organ by tubes constantly increasing in diameter. Now, it is impossible but that the passage of a column of liquid into either narrower or wider spaces than those it previously moved in, must augment or diminish, respectively, the rapidity of its flow. This assertion is founded on a well-known theorem in hydraulics. In the arteries the current is most rapid where it originates; in the veins, where it terminates. The arterial tunics are constantly distended by the blood; the coats of the veins are frequently collapsed. The former are dense and resisting, and react incessantly on the fluid that traverses them; the latter are thin and flabby, and possess only a slight share of elasticity. In the veins you find numerous valves destined to prevent the reflux of the blood; in the arteries no such contrivances are to be seen. The pressure is uniform throughout the arterial system; extremely various in the different parts of the venous. The physical properties of the circulating fluid differ in the two classes of vessels, as well as the character of its movement; in the one the current is rapid; in

the other slow. If you open an artery the blood escapes in a jet, and with a jerking movement isochronous with the pulse; if you divide a vein, the jet, when such exists, is feeble and uniform in its flow. But the two departments of the vascular apparatus differ most widely in respect of the number and capacity of their component tubes. An artery is usually accompanied by one or two veins; these are of much larger calibre than the arterial vessel. But, independently of these *satellite* veins, there are others placed more superficially. Whenever an artery and a vein are found together, the predominance of the latter over the former, in point of capacity, may be easily proved. If you take into consideration, besides, the number of the subcutaneous veins, to which there are no corresponding arterial trunks, you will at once understand that the total capacity of the veins is vastly superior to that of the arteries. This superiority is chiefly apparent in those points of the frame wherein the circulation of the blood is accomplished with most difficulty. Again, the main use of the anastomoses between the superficial and deep divisions of the venous system is to facilitate the return of the blood to the central organ. The arteries, from their position, structure, and the direction of the currents that traverse them, are independent of the majority of causes that slacken the movement of the blood in the veins. I pass rapidly over these points, which are capable of very great development, because I presume you are familiar with them. But do not be mistaken as to their importance;—if we neglect the physical conditions of the vessels, the resistance of their walls, and the degree of pressure of the liquid, any hope of framing a rational theory of the circulation may be at once given up. Its hydraulic phenomena will become so many mysteries; any physiological explanations respecting it, so many absurdities.

Although the pressure exercised by the liquid in the interior of the venous tubes is feeble, we have clearly seen that it does exist to a certain extent. When we applied the instrument to the jugular the mercury rose several millimetres; when we transferred it to the femoral vein the ascent of the mercury was still more evident. The force of impulsion of the blood in the latter vessel appeared tolerably energetic; however it did not reach the amount existing in the arteries. In attempting to explain this diminution in the influence of the heart, we must not forget to take into account the obstacle to the movement of the blood produced in the capillaries by the adhesion of a motionless stratum to their walls. But although this phenomenon has a decided influence on the velocity of the venous blood, still it is far from being of such consequence as might be supposed. The greater capacity of the venous tubes, as compared with the arterial, is the chief cause of the diminished progressive force of the current within them. If you represent by *one* the capacity of an artery, and by *ten* that of the veins that succeed it, it is clear that each vein will only receive a tenth of the force that moves the blood in the artery. The impulse of the ventricle is divided, but not lost, in the venous system. This is so true that when we cause the entire mass of blood conveyed to a part by an artery to return by a single vein, the pressure is found to be very closely equal in the two vessels. We are indebted for the discovery of this curious fact to M. Poiseuille; you shall by-and-by have ocular demonstration of it. I propose, also, trying some experiments in the present lecture on the circulation of the brain and of the head in general. We will first apply the instrument to one of the external jugular veins; once we shall have ascertained the height to which the mercury rises under the influence of the current moving in it, we will compress the jugular of the opposite side, so as to force almost all the blood of the head to return by the vein connected with the tube. It is clear that the column must rise in consequence. The carotids have been laid bare, and a ligature thrown round them, in such manner that we shall be able to suspend and re-establish the circulation through them at pleasure. These experiments, like a variety of others we have performed, will, in all probability, have more than a merely physical interest. They may give us some information on certain pathological phenomena affecting the brain, that have been repeatedly observed, but never yet explained. Thus, among the consequences known occasionally to follow the ligature of the primitive carotid in the human subject, stands cerebral hæmorrhage. A young girl in whom I tied that vessel on the left side, for an enormous fibro-osseous tumour seated in the superior maxillary region, was seized with hemiplegia of the right half of the body the sixth day after the operation. The paralysis diminished subsequently, but the intelligence of the patient remained impaired. This patient presented a very curious peculiarity dependant on the condition of the brain: before the operation she read very well, but since its performance she has lost the power of combining letters so as to form syllables; she is no longer able to read. No one ever entertained a suspicion that, by diminishing the volume of blood which flows to the brain in a given time, sanguineous extravasation into the substance of that organ would be promoted. Nevertheless, my case is not a solitary instance of such an occurrence; you will find seven or eight examples of hæmorrhage induced by ligature of the carotid recorded by authors. Mr. Samuel Cooper has collected several in his "Surgical Dictionary." No satisfactory explanation of these cases can, I think,

be given in the present state of our knowledge. Compression of the carotid arteries has lately been proposed as an excellent method of treating cerebral hæmorrhage. I have never tried it myself, nor am I aware that any other person has done so either, or at least had to congratulate himself on its employment. It is evidently a mere theoretical conception; and however rational it may be in appearance, it is to be feared that experience would not give any evidence in its favour, as we find that apoplexy follows the ligature of the vessels in question. Perhaps we should, in this instance, as in a multitude of others, be employing, without suspecting it, a simply homœopathic remedy.

I told you we should apply the instrument to one of the jugular veins, and then suspend the course of the blood in the other. The hæmodynamometer has accordingly been fixed, and the mercury rises to 15, 17, 15, 16, 17, 16 millimetres. The blood in the jugular consequently presses with a force of 7 or 8 millimetres, as the weight of the subcarbonate of soda holds 10 millimetres of mercury in equilibrium. I now tighten the ligature applied to the other—the left—jugular. Almost all the blood in the head is thereby diverted to the opposite vein; a very trifling quantity will return by the internal jugular veins, which, you are aware, are extremely small in the dog. The column evidently rises; the scale marks 20, 25, 23, 26, 25 millimetres. I loosen the ligature, and the column falls to 15, 16, 15, 17, 16 millimetres. I will now tie the vein and leave the ligature round it. It will be very easy, if we think fit, to cut the ligature, and so re-establish the circulation in the vessel. The mercury rises in consequence, oscillating between 24, 23, 22, 23, 25 and 24 millimetres. The pressure has evidently increased, but the close vicinity of the chest prevents us from obtaining so well marked results as we should were the experiment made on a distant vessel, such as the femoral. It is well known that any cause which lessens the facility of the movement of the blood in the veins, increases the pressure in those vessels by the accumulation of a greater quantity of liquid in their cavity. A ligature placed round the arm will double or treble their size; the erect position, if long persisted in, will, as well as pregnancy, cause large trunks to become apparent where the smallest ramuscule was not previously perceptible. The arteries are, in consequence of the density and resistance of their tissue, infinitely less subject to these variations in point of size; nevertheless, they do undergo such changes. It is impossible to compute, with any accuracy, the respective capacities of the two systems of tubes. The mode of death enlarges or diminishes the diameter of the veins, according as those vessels contain more or less blood. Injecting them would lead to a very fallacious estimate of their relative magnitude; for the veins are exceedingly distensible, and their capacity may become enormous in comparison with what it was in the living subject.

You remember among the preparations I had brought here from the Museum of the Faculty, that of an infant in which the inferior vena cava filled the entire cavity of the abdomen. Yet I had given directions that the best preparations only should be selected, in order to give you, as far as possible, correct notions respecting the size and other characters of the vessels. I shall, for these reasons, make no attempt to estimate, even approximatively, the capacity of the veins. The point we are now anxious to ascertain is, whether the pressure does or does not augment in such parts of the system as are left free, while the circulation is suspended in others. The experiment we have made places the affirmative of the question beyond all doubt. Let us now see what will happen on compressing one of the carotids. By this, it is true, we shall diminish the quantity of the blood transmitted to the brain, but the pressure will be increased in the other arteries that participate in carrying on the cerebral circulation. I now compress the carotid, and the mercury gives 20, 22, 20, 21, 19 mill. I no longer interfere with that vessel, and we have 21, 19, 20, 22, 21 mill. There is, therefore, no appreciable difference. The animal is quite calm, so that the respiratory movements cannot have any very marked influence on the results of the experiment. I now, instead of compressing the carotid with my fingers, tie that vessel. If you had not seen me tighten the ligature, you would not have known, by any effect produced on the mercurial column, that I had done so, for it stands between 20 and 22 mill. No notable increase of pressure occurs except during deep expirations. The sudden rise to 40, 45 mill., that has just taken place, is caused by some struggles on the part of the animal to get loose; these have now ceased, and you see that the column again oscillates between 20 and 22 millimetres. When the left carotid is compressed the vertebral arteries alone supply the brain with blood, and the pressure supported by the coats of the latter vessels will be materially augmented. Will this increase of pressure suffice to counterbalance the effects of the diminished volume of the liquid? The comparative diameter of the vertebral and carotid arteries renders this hardly a likely supposition, but we will try the experiment. You remember that we have already tied the left external jugular vein, and the right carotid artery. I compress the left carotid, and the instrument gives 15, 17, 14, 16, 15, 16 mill. I allow the blood to pass freely through it now, and the mercury rises to

20, 22, 19, 21, 20, 22 mill.; the column, therefore, evidently fell under the influence of the compression; but, to obtain additional certainty of the correctness of this result, we will repeat the experiment. The respiration is easy and gentle, and the animal very rarely makes any effort to disengage himself from the cords that bind him down; we may, therefore, feel secure as to the accuracy of our results. I again compress the vessel; we have 14, 16, 17, 15, 17, 15 mill., and on ceasing the pressure you see it rises to 21, 22, 20, 23, 19 mill. It is, therefore, incontestable that the compression of the left carotid determined a notable fall of the column. Of this experiment we shall hereafter have occasion to avail ourselves in the consideration of certain physiological phenomena connected with the cerebral circulation. Now, it is certain that, as a general proposition, the pressure cannot increase in an artery without augmenting at the same time in the veins; but we must not forget the relations of the vessel on which we experiment, as they may modify the results obtained. The ligature of the carotid in the animal before me must have increased the pressure in the vertebral arteries; yet, notwithstanding this, the mercury fell in the tube applied to the jugular. The most plausible explanation of the difficulty seems to be this: when the circulation is suspended in the carotids, the blood is conveyed to the brain by the two vertebral arteries only,—vessels which are considerably smaller than the former pair. The quantity of blood carried to the brain by the vertebral arteries, under such circumstances, is no doubt greater than that usually traversing them, but it is less than that reaching the brain in the ordinary state of things.

Had I applied the instrument to the femoral vein, and then tied the carotid arteries, the mercury would undoubtedly have risen several millimetres. In the one case we modify the progressive force only of the liquid in the vessel subjected to experiment; in the other we modify its quantity. The increase of pressure effected in one way is lost in another; as the cause is complex the effect cannot be simple. But, if we even suppose the velocity of the liquid to be such as to supply the deficiency of space in the vessels, and if we grant, further, that the blood conveyed by the vertebral arteries equals in quantity that normally sent to the brain, we shall not even yet have the physical conditions necessary to cause any increase in arterial pressure, to be felt to the same amount in the veins. The reason of this is plain: the capillary vessels, whereby the communication between the veins and arteries is effected, form a vast and intricate rete, each cylinder of which bears, as I before said, about the same ratio to a vascular trunk as a fibril of the root of a tree to its trunk. So long as it is contained in the large trunks and their principal branches, the liquid moves without difficulty, but, when arrived in the capillaries, the obstacles to its passage become serious. The chief of these are the increase of capacity of the vessels themselves, and the motionless stratum of serum adhering to their walls. The narrowness of the cavity available for the circulation is so great that a single globule only can frequently pass through it; and that single globule requires to be altered in shape for adaptation to the area of the canal. Now, these circumstances exercise a very powerful influence on the degree of pressure supported by the veins.

I now proceed to ascertain, experimentally, the progressive force of the blood in a superficial vein of the extremities. The vena saphena interna is laid bare; its small size renders the introduction of the tube of the instrument a matter of some difficulty; however, it is now fixed. The animal is, as you perceive, both strong and restless, circumstances which may interfere with the success of our experiment. The mercury marks 20, 22, 19, 22, 20 millimetres, showing that the pressure is very feeble. I now seize the animal's paw in my hand, so as to suspend the circulation in the deep vein, above the point where the tube is applied. The mercury rises to 25, 28, 30, 32 millimetres. I now no longer press the limb, and the column falls to 19, 21, 19, 20 millimetres. Nothing can be more simple than this; my hand acts on the limb of the animal as the bandage applied to the arm of a person about to be bled, a greater quantity of blood consequently accumulates in the veins, and the pressure is proportionally augmented. I next proceed to place the instrument in the femoral vein; the resistance of its coats shows that the pressure within it is greater than in the saphena. I have already had occasion to point out this difference in the properties of the subcutaneous and deep-seated veins. The tube is now introduced with its point towards the capillaries. I remove the straps from the animal's limb in order to render the circulation perfectly free. The cock is now turned, and the scale marks 55, 60, 63, 58, 60 millimetres; the pressure is, therefore, very nearly arterial in force. This is an important fact; it explains why a wound of a deep is a much more serious injury than that of a superficial vein. The latter class of vessels are rarely the seat of dangerous hæmorrhage; bleeding from the former may, on the contrary, be very abundant and difficult to stop. I say that this result is very important, and highly interesting to the surgeon and physiologist. I delight in associating the two names; to separate them designedly is, indeed, an insult to common sense; for the habit of ope-

rating on animals gives so much precision to the hand, that when any one so habituated is summoned to act on the human subject, instead of encountering a difficult task, he feels it to be a sort of manual recreation.

In order to estimate comparatively the force of progression of the arterial and venous columns of blood, it becomes necessary to cause the blood driven by the ventricle through a single artery, to return to the heart by a single vein. This has been done by M. Poiseuille; after having isolated the femoral vein and artery he suspended the circulation in the thigh by means of a ligature tightly applied to the limb; and he then ascertained that the amount of pressure was the same in the artery and vein. This experiment, as far as regards the operative part of it, is precisely the same that I performed long since for the purpose of demonstrating the mechanism of absorption; I even separated the thigh completely from the trunk. In the present case circular constriction of the limb is quite enough. The preparatives of the experiment are all arranged; the femoral vein and artery are laid bare, and both enclosed in a noose, which will serve to lift them up if necessary. You see that the mercury still oscillates between 55 and 60 millimetres, which indicates a very considerable degree of pressure for a vein, and is explained by the anatomy of the vascular system of the thigh. The venous trunk on which we act, carries back of itself alone almost all the blood conveyed by the femoral artery to the periphery of the limb. The remaining branches that concur in maintaining the venous circulation are either tegumentary or anastomotic, and of small calibre. I now obliterate the cavities of those branches by forcibly tightening the two ligatures. The scale marks in consequence 80, 85, 75, 85, 80 millimetres. This is almost exactly the amount of pressure indicated in the artery also; the pressure supported by the latter is, therefore, transmitted nearly undiminished to the vein. I loosen the ligature, and the mercury falls again to 62, 55, 58, 60, 58 millimetres, in other words, to its normal level.

I shall now interrupt the circulation in the artery; that of the limb will still be carried on to a certain extent, for I have laid bare the vessel below the origin of the profunda femoris. Besides, there are numerous anastomotic branches, which bring the arteries of the pelvis into communication with the muscular ramusculi of the thigh. I now compress the artery; the column falls from 62 millimetres, at which it stood, to 55, 50, 45, 42, 35, 33 millimetres. Now, that I have ceased to compress it, the mercury rises to 62, 60, 63 millimetres. I again compress, the instrument forthwith marks 55, 48, 45, 40, 36 millimetres; I let the vessel go, and we have 58, 60, 58, 62 millimetres. The venous and arterial circulations are, therefore, intimately connected; whatever acts on the one, acts at the same moment on the other. Any influence on the part of the capillaries is sought for in vain; powerful in books, it is powerless in nature; nay, further, it is shown, by observation, to slacken rather than to accelerate the progression of the blood. In truth, it is not the capillaries that are busy in the matter, but the imaginations of physiologists.

One of your number, Gentlemen, who is attached to the Venereal Hospital, has done me the favour to bring me a specimen of the blood of an individual affected with constitutional syphilis. Examined under the microscope it presented two classes of globules; those of one class were of ordinary form and size, those of the other much smaller. However trifling the importance of the latter may be, still their existence deserves to be noted; I would, therefore, beg of the gentleman who brought me this blood to procure me that of some other individual labouring under the same disease, in order that I may ascertain if the presence of this smaller species of globules in the blood of such subjects, be a constant or a merely fortuitous circumstance. The serum and clot were in this blood in the ratio of 11 :30. The clot is soft and diffluent. I must here inform you that the method I fixed on for determining the relative quantities of serosity and coagulum (which consists, as I have already informed you, in submitting the clot to pressure), is fallacious in many cases; we must, therefore, endeavour to devise some plan more certain in its operation. Meanwhile I proceed to communicate to you the circumstances of a very interesting case. The blood in these two glass vessels has been taken from a man affected with Bright's disease,—a complaint characterised by a peculiar disorder in the functions of the kidneys.

The urine voided by such patients is remarkable for containing a considerable quantity of albumen, which may be coagulated by the addition of a few drops of nitric acid. But, before going further, let me point out another illustration, in addition to those already given, of the error into which we may fall, if we trust to inspection alone, as a means of ascertaining the proportional quantities of the serum and clot. The blood in one of these vessels contains 17 of fluid, 62 of solid elements; that in this other, which is of the same nature, in as far as it comes from the same individual, and was drawn at the same time, contains a large share of serosity. It is evident that the coagulum in the former case must contain much serum imprisoned in its substance. It seems to me we might acquire a more accurate notion on this question by first weighing the blood, and then gradu-

ally evaporating the serum. This operation would, however, take up at least twenty-four hours, and consequently would be too slow; for the future, therefore, I will take a slice of clot, of fixed size, and weigh it; and as the mass acted on will be small, its dessication will, of course, be more speedily completed. We must not forget, however, that, by proceeding in this manner, we shall have the solid elements of the serum united with the clot. But to return to the characters of the blood in my patient labouring under *albuminous nephritis*. The ordinary globules of the liquid were easily discoverable under the microscope; but, in addition to these, I detected a multitude of little globules, or rather corpuscula, without any determinate shape, which I might almost affirm were formed of albumen. I then proceeded to a slight chemical examination of the serum, being especially anxious to learn if the serum of this blood would, as in ordinary cases, coagulate by the action of heat. It did form into a mass under the influence of that agent, but when coagulated it resembled pus much more strongly than healthy serum; besides this, an albuminiform liquid oozed from the clotted mass, which I was unable to succeed in coagulating. The solidified part, too, is less firm and coherent than usual; for, as you perceive, the glass rod sinks into it simply from its weight. This examination, imperfect though it be, seems to indicate the reality of a morbid alteration of the albumen of the serum in the disease under consideration; hence we need not be surprised at finding that element in the urinary secretion. It remains to be determined whether the albumen carried off with the urine has the same properties as that we have just examined. I will take the first opportunity of inquiring into this.

From the first moment I set about these researches on the blood I perceived that the simplest questions were far from being thoroughly understood. It has, no doubt, been long noticed that the blood is unusually aqueous, or contains an unusual share of serosity, in persons of lymphatic temperament, in chlorotic patients, and in those subject to passive hæmorrhage; but the really important point to be established is the proportion in which the elements must be combined in order to give rise to those pathological conditions. This it is which we should strain all our energies to ascertain; the mind recoils, I admit, before the extent and variety of research that such an undertaking demands, and, so far as the trials we have ourselves made, go, I must acknowledge that little has been discovered respecting the precise proportion of serum and coagulum which is incompatible with life. We constantly encounter the most unexpected obstacles. We commenced by receiving the blood into a graduated glass vessel, and noticing the respective heights of the serum and clot. I fancied this was an excellent plan of investigation, but I soon learnt I was in error, by finding that a very considerable mass of serosity might be invisible from its being contained in the substance of the coagulum. This morning we made trial of a new method, which has, much to my regret, proved equally unsatisfactory: I put two parts of water into a glass vessel, and added a third part of venous blood; the mixture coagulated completely, and a few drops only of serum subsequently transuded through it. Again, here is some blood taken from a woman, aged 77, affected with pneumonia. It is true that the disease is not very acute; no trace of serum is here visible, consequently, were we to decide by appearances, we should declare that this blood scarcely contains any serosity at all. Let not these difficulties discourage you, Gentlemen; every experiment we make, though it fail in the end proposed, teaches us something new; our time is never, therefore, wasted.

You remember that, at our last meeting, I injected ten ounces of human serum into the veins of an animal. He died forty-eight hours after the operation, and presented, on post-mortem examination, the following lesions:—His blood was evidently altered in properties by the serum injected; the traces of its disorganisation were so manifest, and its fluidity so remarkable, that on opening a vein in the neck after death, and hanging up the animal by the hind paws, the greater part of the blood contained in the vessels trickled away; the remainder contained a few imperfectly-formed coagula. It is probable that the fluidity of the animal's blood is to be explained by the alkaline character of the serum introduced; the lesions of the pulmonary organs were not very strongly marked, which accounts for death not having been instantaneous; but the most remarkable morbid condition discovered by the autopsy was, beyond question, that of the cephalo-rachidian fluid; it was, in the first place, reddish in colour, as though the globules of the blood were dissolved in it; in the next, it had formed into a sort of coagulum, and seemed of a totally different nature from the normal liquid. I am inclined to think that the albumen of the serum transuded through the capillaries of the pia mater, and so became extravasated into the sub-arachnoid cavity. The injection of simple water into the veins does not determine this gelatiniform appearance of the extravasated liquid. I remember that, in some experiments made a long while since, in which I injected water into the veins, I produced various disorders in the action of the nervous system, such as general trembling, involuntary movements, signs of coma, and various forms of tetanus. I further recollect that I detected

disorganised blood under the inner lamina of the arachnoid in the animals on whom those essays were made. This lesion alone would, as it did in the case adverted to in my last lecture, suffice to produce the most serious disorder in the functions of the brain. Now, were I one of those who servilely follow the favourite doctrines of the hour, I might have presented this to you as a superb case of *meningitis;* but tell me, I pray you, should I have thereby thrown any new light on the facts we have observed? None, absolutely none; I infinitely prefer, therefore, stating to you the simple fact, that the blood became extravasated because it had lost those physical and chemical properties which endow it with the capability of circulating through the minute canals of our organs. If we recollect, too, that this animal presented the signs of a lesion of the nervous system, we may easily deduce the conclusion, that its state of prostration, and the frequent contractions of its limbs, were induced by the morbid condition of the cephalo-rachidian fluid; the animal's right eye was seriously affected, and had already commenced to suppurate. But there is another important lesion in this animal to which I wish to direct your attention; I mean the marked disease in the intestinal canal; I mean these patches in a commencing state of ulceration, which, had the disease been of longer duration, would have passed into the state of true ulcers; I mean, too, the tumefaction and engorgement of the mucous tissue, caused by the effusion of blood, and accompanied by all the phenomena which we see manifested in the diseases known by the name of typhoid affections. When a pathologist of the present day opens a body and finds the external surface of the intestine maculated with livid violet, or bluish patches, and, on cutting into that intestine, ascertains that its internal tunic is strewed with reddish elevations, formed by swollen follicles in a state of ulceration, and by laminæ of a sort of albuminous matter, he feels perfectly satisfied; there is no need, he conceives, to push the examination any further, for these patches and these follicles are the actual seat of the disorder; the inflammation spontaneously developed in them explains, in the most perfect and simple manner, the whole train of phenomena observed. But, admitting that inflammation has something to do with them, let me ask him, what was the cause of that inflammation? To this he will not easily find an answer; on the contrary, he will be forced to confess that he has given a ridiculous name to a still more ridiculous idea,—an idea without a particle of solid foundation to rest upon, and one of which all the positive knowledge we possess tends to demonstrate the complete fallacy. What is explained by this word inflammation? He not only gives us no additional knowledge by its use, but actually throws a thicker veil than before over the nature of the case; he appears marvellously learned in the eyes of the vulgar, no doubt, but, in reality, he is ignorant of the essence of the disease, treats it according to erroneous theories, and, in consequence, frequently induces serious complications. Will he affirm that the intestinal lesions are primitive? He will fall, if he do, into an eggregious error. Do you fancy that if we had put this animal to death before performing the experiment on him which led to the fatal result, we should have discovered the intestinal lesions we now see before us? Indubitably we should not. How, then, it remains to be inquired, was the affection produced? We acted on the mass of the blood by the injection of serum into the veins of the animal; that liquid was alkaline; it liquefied the blood, which, consequently, became unfit to circulate in the infinitely small vessels of the intestinal glands or follicles; its elements separated, the liquid part of them became extravasated by exbibition, and the solid portion distended and burst the delicate tubes it was unable to traverse. Is all this inflammation, or a train of most simple physical phenomena? Can you wonder if I adopt the latter opinion, when it explains, *seriatim*, the lesions we observe, and the manner in which they were produced? The same explanation is applicable in the case of the mesenteric glands, which are only in a commencing state of alteration, because death occurred in forty-eight hours; they, nevertheless, present the incipient stage of sanguineous effusion brought about by the same cause. I know full well that the doctrine I have laid before you is utterly opposed to the ideas admitted by the majority of pathologists; but against the experiments I have performed, directly illustrating the question at issue, they can have no valid objection to urge. Those experiments show the emptiness of the classification of fevers into entero-mesenteric, ataxic, typhoid, &c., which were established at the expense of so much labour, and which, while they appeared to benefit the science, in reality increased its perplexity, by creating imaginary distinctions between phenomena originating in the same causes and producing the same effects. What is, perhaps, most to be regretted is, that meritorious men should have engaged in laborious researches respecting these fevers, previously imbued with such notions as have rendered their labours, conscientious though they be, worse than useless.

But, Gentlemen, let me once again assure you that it is not my wish to refer every disease to a morbid condition of the blood. I disavow the doctrine, and protest, in the strongest manner, against such a view being taken of that which I really profess. You see that I allow myself to be led wholly by

experience; I have no ambition to advance a single step beyond what it teaches us, persuaded as I am, that in adopting experience as my guide, I have fixed on the surest method of avoiding the errors, absurd in their essence and disastrous in their consequences, into which theorists so frequently fall. Such experiments as that we have this day discussed may become extremely useful in therapeutic applications. Thus, when the cholera raged in England, the practitioners of that country, finding their efforts to save their patients on ordinary principles invariably fail, fancied, like many others, that the disease consisted in a loss of the aqueous part of the blood, which they supposed was carried off with the vomited matters and alvine evacuations. Acting on this theoretical notion,* they attempted to remedy the flux of serosity by the injection of artificial serum into the veins, and actually announced their having obtained marked success by the plan. I had, also, though with different motives, employed the same method in desperate cases: I used an artificial serum prepared by M. Persos, but I was not fortunate enough to save a single patient. I am, therefore, of opinion that, if the English medical men were successful, the favourable result must have depended on their patients not having been in so desperate a state as those on whom I practised injection of the liquid alluded to. But, whatever be the state of things as regards those facts, it is now clear that such essays on the human subject would no longer be allowable, because we have reason to apprehend similar results to those observed in the animal into whose veins we injected human serum. Here, then, is another fact of which no suspicion could have been entertained before our experiment; let me counsel you to meditate upon it seriously; it will serve to show how cautious we must be about admitting the propriety of applying any speculation, however specious, to the practice of our art. For my own part I shall always prefer, no matter how afflicting the spectacle may be, leaving a patient to the simple resources of Nature, to employing any process, which, though useful in appearance, might, in reality, produce the most baneful effects. The question under discussion bears, as you perceive, on the most serious disorders to which the frame is liable, and is, therefore, of the highest order of interest. But, before coming at its solution (if, indeed, such an honour be really in reserve for us), that is, at a precise knowledge of the proportions of serum and clot which constitute normal blood, apt for life, we must make a comparative study of the albumen, fibrine, and other elements of that fluid. To these points I will direct your attention in my coming lectures.

* M. Magendie has here fallen into a grievous error. The proposal to inject a solution of saline matter into the veins was not founded upon *theoretical notions*, but precisely on his own principles of *experimental* research. Chemical investigations had demonstrated the absence of serum and salts in the blood; the absent materials were discovered in the dejections; hence the idea of restoring to the blood, by direct injection, the materials which it was rapidly losing, in consequence of the disease.—Ed. L.

PERSISTENCE OF LIFE
DURING EXTENSIVE
DISORGANISATION OF ONE HEMISPHERE OF THE BRAIN.

To the Editor of The Lancet.

Sir:—I shall feel obliged by an insertion of the subjoined case in the columns of The Lancet. I am, Sir, your obedient servant,

George Lowther, M.R.C.S.

Kingston-upon-Hull,
Nov. 24, 1838.

On Thursday evening, the 11th of October last, I was sent for to attend Thomas Lang, æt. 33, of regular habits, and phlegmatic temperament. I found him presenting the following symptoms:—Insensibility (incomplete, as he possessed the power of giving indistinct replies to questions); oppressed respiration; pulse ranging between fifty and sixty, moderately full and hard; restlessness, increased by the slightest exciting cause; lower extremities cold; tongue moist, and bowels constipated; urine not retained; in short, each symptom existed of a comatose condition, rapidly verging upon confirmed apoplexy. By queries addressed to the attendants I elicited that my patient had been amaurotic for seven years, supposed, and not injudiciously so, to have been remotely occasioned by a blow upon the occiput with a hammer. He had had occasional attacks (rendered more frequent as his vision became more impaired) of epilepsy, accompanied with violent shooting pains in the forehead. On the Saturday preceding, while at breakfast, he was suddenly seized with a slight epileptic convulsion, of short duration, and succeeded on the following day by another. He gradually became worse, and the state which I have above described at length supervened.

It is superfluous to say what my prognosis was; of course none other than a fatal result could be anticipated; however, I directed a large blister to be applied to the nape of the neck, hot applications to the feet and legs, and the bowels to be opened by the common cathartic mixture.

Oct. 12, morning. No apparent alteration in any of the symptoms of last evening; the cathartic has failed in unloading his bowels.

Ordered him an enema to be injected immediately.—Evening. I found my assistant had only partially succeeded in injecting the clyster in consequence of the powerful resistance of the patient. I renewed the effort with no better success. Twelve leeches to be applied to the temples, and a wineglassful of the cathartic mixture to be taken every hour.

13, morning. More apoplectic; consciousness has entirely left him. The leeches he tore away almost immediately, therefore very little blood was abstracted: the pulsations at the wrist have fallen in frequency, varying between forty and fifty; tongue moist; skin cool; bowels have been slightly moved. To have two minims of croton oil immediately, and a gently stimulating ammoniacal mixture every third hour.—Evening. The stimulant has not raised the heart's action; the radial artery now pulsated steadily at forty; bowels have been copiously evacuated; urine escapes freely; a little wine and water to be administered occasionally. Continue the hot applications to the feet.

14, morning. Pulse forty, and full; other symptoms the same as last evening. I now, for the first time, ventured, very cautiously, to abstract blood from the arm; a few ounces had flowed, perhaps three, the pulse fluttered and became imperceptible, an accelerated circulation, however, was the result; the stimulating mixture to be repeated.—Evening. Pulse forty-eight, and regular. The bleeding has produced no beneficial effect upon the brain; his restlessness has subsided; respiration quick, and laboured; hot applications to the feet, wine and water, weak, to be continued occasionally.

15, morning. Insensibility continues; pulse forty-eight; tongue moist; skin cool; stools and urine pass off freely. I again abstracted three or four ounces of blood; the pulse fluttered, and fell as before; no salutary effect followed. Continue the stimulants.—Evening. Pulse from fifty to fifty-four; in other respects, no change since the morning; about three ounces more blood were abstracted, with the usual result.

16, morning. Pulse has risen six beats; with this exception the other symptoms are as heretofore. Continue the croton oil; let him have one grain of calomel every three hours.—Evening. From this time, it was evident that my patient was rapidly sinking, and, on the morning of the 19th, I found him moribund. He expired about four in the afternoon.

Autopsy, seventy-two hours after Death.

The calvarium having been raised, and the dura mater reflected, the vessels of the cerebrum, together with the various sinuses, were found, as might have been expected, in a high state of turgescence. The brain was examined in situ, in order to prevent any rupture that might otherwise have occurred. Without the aid of the knife it was easily discernible, *à priori*, that the anterior lobe of the left hemisphere had undergone very extensive disorganisation; in fact, so great was the lesion, that its superior surface presented a perfectly transparent aspect; serum was seen through its structures, and, by gently raising the head, the convolutions of the cerebrum were made to undulate nearly throughout the extent of the hemisphere. I punctured the transparent part, and out flowed serum in abundance. The lateral ventricle was almost obliterated, the medullary substance of the anterior, middle, and part of the posterior lobe being extensively destroyed; and when it is said that serum formed nearly the only centrum ovale minus, we may be better enabled to form an estimate of the quantity. The whole of the medullary substance remaining was coated with a thick layer of pus, and at the base of the ventricle, immediately over the thalamus, a tumour was situated, resembling, in size and appearance, the eye of an ox, with what might be compared to the transparent cornea, directed upwards. This diseased formation, was partly imbedded in a thick layer of purulent matter, and appeared to have very slight, if any, connecting media with the subjacent structure, being readily removed with the forceps. The right hemisphere was next examined: by making an incision perpendicularly downwards, and separating the edges with the fingers and scalpel, the ventricle was found to be greatly distended, containing, at the lowest computation, not less than six ounces of serous fluid; there was no disorganisation of structure, and, with the exception of the effusion, the hemisphere appeared to be perfectly normal; the nervi optici, with their thalami, had shrunk considerably: the cerebellum was quite healthy.

Remarks.—I cannot think the foregoing case will be considered uninteresting, either by the physiologist, or the pathologist. The post-mortem appearances are so intimately allied with, and so satisfactorily explanatory of the functional derangement which had existed for a long time prior to, and more particularly towards the close of life, that, viewed in this respect alone, it is characterised by features not altogether undeserving of attention. An examination disclosed a tumour in the left ventricle. The patient's friends admit that deceased had received a blow over the occipital region with a heavy instrument. At no distant period, symptoms of defective vision arises, attended with violent pain in the forehead. Presupposing the cause to be equal to such an effect, it is fair to infer, that the blow on the head was the immediate cause of the morbid growth; the tumour being situated over the thalamus, sufficiently explains the loss of sight, inasmuch as the disease appeared to

influence the attacks of epilepsy, and the gradual decay of vision; for, in proportion as the tumour increased, in the same ratio the former became more frequent, and the latter more aggravated. Innumerable are the instances on record where the respiratory function was maintained through the instrumentality of one lung; and others, where a single kidney has discharged the excretory office of both kidneys. Supposing the brain to be the organ through which the mind operates, we have here an example that two-thirds of one hemisphere may disappear, and yet the intellectual faculties remain unimpaired. It would be absurd to suppose that the extensive disorganisation discovered in the left hemisphere had occurred during the last week of the patient's life; the symptoms of his illness forbid such a supposition. The immediate cause of death is unquestionably attributable to the effusion of serum in the opposite side of the brain. Insensibility became confirmed on about the 13th, and a few days afterwards vitality was extinct.

The inference deducible from the foregoing facts is this, that disease in one hemisphere only is not incompatible with the continuance of physical and intellectual life; but if both are affected simultaneously, vitality ceases to exist. Whether we interfere here with the tenets of the phrenologist I will not hazard an opinion. To pursue the subject further would lead us into the province of the metaphysician,—a field which is far too speculative for the space allotted to these brief remarks. My principal object throughout has solely been to give a plain statement of a few pathological facts.

RELIEF OF CONSTIPATION
OCCURRING IN
INSANE PERSONS.

To the Editor of THE LANCET.

SIR:—In the account given in a recent Number of THE LANCET of the inquiry into the cause of the death of the pauper lunatic, Mrs. Strong, it was stated in the evidence that the poor woman had suffered from constipation of the bowels of an obstinate character, insomuch that drastic purgatives, croton oil, &c., were declared needful, and habitually given, to procure evacuations. As this is a symptom *common* to many lunatics, and the proper treatment of it not always understood, as it is frequently the source of much misery to such patients (for from this cause, I infer must have arisen, in the above-named case, most of the beating, washing, scrubbing, exposure, &c., as detailed in the evidence, for all this seems to imply that the patient was frequently in a state of filthiness, for which the purgatives, croton oil, &c., would account), I am induced to transmit for publication in your valuable Journal, if deemed worthy, a short account of the simple method of treatment which I have invariably found successful as regards the symptom above-named. The following case is an example:—

Miss M., aged 30, about two years since came under my care, suffering under mania and total incoherency of ideas; she had been deranged seven years, five of which she had spent in a County Asylum. When her distressed father brought her to my establishment, he did not fail to bring with her a full box of cathartic pills, earnestly requesting us, on no account, to neglect the bowels, which never acted for years without her taking strong pills; moreover she was frequently subject to "prolapsus ani." In a day or two after her arrival the rectum, instead of evacuating its contents, came down; she had taken no pills; she kept her bed; the bowel was returned, and the following mixture was prescribed:—

Comp. infusion of roses, ℥vj;
Comp. tinct. of cardamoms, ℥ij;
Sulphate of magnesia, ℥ij. To take a wineglassful every second hour until an evacuation be procured. In a few hours the bowels were relaxed; there was no displacement of the rectum; she afterwards, occasionally, took a dose of this mixture before breakfast, which never failed to operate. From that time to the present she has never suffered from prolapsus ani, and scarcely ever requires any aperient, the bowels acting spontaneously and with great regularity; her general health has become much improved. The bowels, in this case, appear to have become mechanically disarranged, and the prolapsus ani had been the result of long habits of costiveness and the frequent use of drastic aloetic purgatives. By the astringent action of the infus. of roses, the stomach and bowels became braced up into their proper position, and their calibre reduced to more natural dimensions, which effects, being aided by the purgative power of the Epsom salts, at the same time urging forward and evacuating their contents, the cure of this generally very perplexing ailment was effected.

Mr. J., a single gentleman, was about forty years of age, possessed of a competent fortune, pursuing no business or regular employment; his habits were those of the man of pleasure; he was of a boisterous, joyous, and somewhat domineering disposition.—About two years ago he went on a journey to Boulogne, in France, by way of varying the monotonous course of his life; and having indulged, on board the packet, too freely in drinking, and following up the practice on his arrival at Boulogne, he was suddenly seized (in an interval of depression consequent upon the state of over-excitement) with a mental panic; he imagined the French police in pursuit of him, for supposed crimes against the Government, and, in a distressed

state of mind, he came homewards; but his fears did not leave him; his enemies were now on board, and pursuing him to England. There he encountered them at all points; on landing at Dover; on his arrival in London; at an inn there he barricaded his bedroom door; then, in an agony of despair, he rung up the waiter, admitted him, and taking his purse, offered it, with all its contents, if he would contrive for him the means of escape from his pursuers, whom he believed to be surrounding the house to ensure his capture. At length he got placed on a coach, which was to convey him to his home in N———shire, from which he contrived to escape, and was found a few miles from his native place, in the fields, late in the evening. At home he found no peace; every person who approached his house was to his affrighted imagination, an officer of justice coming to arrest him for supposed crimes. He was removed to a farm-house, and sedulously attended by a respectable medical practitioner for some weeks, during which the greatest difficulty was experienced in inducing him to eat, for he believed the food offered him was poisoned (a common symptom in lunacy), or to procure action in his bowels, which were obstinately costive: croton oil, and other most active purgatives, were given by force, and enemas administered; but as the difficulties were great, and no improvement took place, his friends placed him under my roof. From a hale and corpulent man, he had already become thin, of a dirty-yellowish complexion, with an expression of anxiety and melancholy. There are two points in the treatment of this case to which I am, on this occasion, desirous to confine my remarks: the method employed to overcome his repugnance to food, and the way in which the bowels were regulated. As to the first, it was obvious that he refused food, not from wanting appetite, but because he believed the food to be poisoned; he was, therefore, taken to the cow-house at milking times, and he had reason sufficient to draw the inference that the milk could not have been poisoned before it was drawn from the cow, and being provided with a large cup he received it full of new milk, and drank it off without hesitation. In this way he principally subsisted for some time; he then grew bolder, and being in the habit of seeing the rhubarb growing in the garden, he was prevailed upon to eat some of this baked; he got on to boiled rhubarb pudding, and he had one daily for himself, as his principal meal, for several weeks. He took, at last, to bread, cheese, and a glass of ale, but was many months before he would take animal food of any other kind. There was considerable difficulty for some weeks with regard to the bowels, from his refusing to swallow medicine, and ten days, or even a fortnight, would sometimes elapse without an evacuation. Giving medicine concealed in food was objectionable, as, should he discover the cheat, his suspicions of an intention to poison him would be strengthened, and his repugnance to food also. At length I one day (and he appeared to be in a more than usually favourable mind) presented him with a tumbler nearly full of a mixture of compound infusion of roses, holding in solution about an ounce and a-half of sulphate of magnesia. He looked inquisitively at the compound; its colour and transparency were something new to him in the way of medicine. I urged its efficacy; he hesitated a few seconds, then, taking the glass, drank off its contents. The action of the medicine on his bowels was exceedingly beneficial; he felt greatly relieved, and did not afterwards refuse this medicine when needful for him. In the course of a few weeks his bowels were perfectly regulated, and required no aperients, they performed their functions spontaneously and orderly. After this he gradually amended; he was accustomed to accompany me in long walks, shooting, or riding; a horse belonging to him was kept here for him; or he rode in a phaeton, and visited, occasionally, some neighbouring town, or joined in country sports. His repugnance to animal food gradually subsided, and in about fifteen months he perfectly recovered, and is now in the full enjoyment of his bodily and mental faculties, his property, home, and friends.

I hope the method of treatment I have detailed, of that symptom so common to the insane, may not prove uninteresting to your readers (I mean torpidity of the bowels and obstinate costiveness), and that it may be useful to many an unfortunate, now, perhaps, undergoing the pains and penalties of violent drastic purgatives, croton oil, enemas, &c., after the same fashion as that which poor Mrs. Strong endured. I remain, Sir, your obedient servant,

George Bodington, Surgeon.
Driffold House Asylum, Sutton Coldfield,
Warwickshire, Nov. 19, 1838.

ON THE

TREATMENT OF FRACTURES

WITHOUT

SPLINTS OR TIGHT BANDAGES.

By W. C. Radley, *Esq., Surgeon, Newton Abbott, Devonshire.*

(Continued from page 298.)

"Whatever is right in principle is wrong in practice." Thus wrote a political sophist, Arthur Young, some 45 years ago, in French revolutionary days: and, looking at the prevalent practice in fractures, one would almost conclude that medical men are satisfied to square their conduct by

this precious axiom. To explain. Every well-informed surgeon who attempts to bring the fractured ends of a bone into accurate appositeness, first, for obvious reasons, so bends the limb as to relax or shorten the more important muscles. No one, who has seen much practice, will offer disputation on this point, because he must, for instance, have observed that, with all possible tact on the part of the surgeon, reduction has been impracticable until, a protruding extremity of the bone having been removed by the saw, the fractured ends have been nicely adjusted. This state of things it is of the first importance should be well done. Without it no subsequent care will avail, and if early neglected, no after-thoughts of emendation can be accomplished without much pain to the patient.

Where a happy reduction has been with some pains effected, by proper muscular relaxation, what is the next thing done? The limb, from the bent position aforesaid, is then, with more or less force, brought by the surgeon to some constrained line or plane, which will allow of this or that splint or apparatus which he most patronises to be applied. It will at once be seen that if those muscles whose contractile force he has endeavoured to elude, have any power left, they are now likely to exert it.

Now, the first act in this surgical proceeding is in close conformity with the *principle* of muscular relaxation.* The second step becomes a direct violation of that great principle in fracture, its physiology, and so every future scene is conducted to the end of the piece.

Yet medical men have applied *the principle* in the first act, and departed from it *in their practice* in the second, continually. Even critics have vauntingly set up their dicta on this point. Some anonymous person, no matter who, in the "London Medical and Surgical Quarterly" for May, 1836, has given us a morceau of this sort. With complacent self-opiniation, he says,—"We are quite satisfied, for our own parts, of the superiority of the plan of *extending* fractured limbs, over that of humouring and bending them. Of this we are quite satisfied." This is the opinion of a London Quarterly. And I, W. C. Radley, of the quarter of Newton Abbott and Newton Bushel, in Devon, feel satisfied that to humour and bend them, is the natural and most proper mode; and that extending them is not the way to mend them quickly, nor can they be so repaired at all without much loss to the patient. But more recent facts will throw "new light" on the subject; and, in future, even metropolitan quarterlies may be inclined to allow the truth—for the details cannot be controverted—that as good cures are attainable by the splintless method as by extension.

Case.—Mr. Bickford, a farmer, residing in the village of Preston, had a son Robert, who was a little more than four years old in October last, twelvemonth. One night I was desired to visit this little boy, on whom a hurdle had fallen, and hurt his thigh very severely. It was late at night, and I found that the father, by placing the hurdle against a wall, had made a sort of ladder of the bars, to enable him to reach some sparrows under the thatch of an outhouse. The little boy climbing under, the accumulated weight brought all down, with one of the oak bars across the child's thigh, which was transversely and badly fractured, with a flake of bone that could be moved up and down, just above the inner condyle of the femur. The limb was swollen very much, and the tenderness extreme.

While the bandage was being made, and the pillow adjusted, the thigh was bathed with a weak lukewarm solution of superacetate of lead, with which the bandage was soaked, and applied lightly on the limb. The pillow supporting the thigh, leg, and ankle, was tied by loops. The little fellow roared aloud, and could not be pacified, and seemed determined not to submit to the new situation of his limb, looking with dread on every one around him. I left him crying and sobbing.

Next day, 6th Oct. His mother informs me that Robert passed a most restless night; he slept but little, as the limb kept twitching (with spasms), so that he cried out when

* For the sake of fair argument I have reasoned with the splint-users in their own way; not that I mean, for one moment, to say that it is for the sake of the *muscles themselves* that we shorten their dimensions or relax them. No. It is to remove pressure or tension from the nerves which supply those parts, and excites them to hostility in attempts to reduce a dislocation, or to set a fracture, that we relax. It is the nervous influence that we dread, when, in the agony of the patient, nerves remote from the part affected sympathise, and the organs of voice, and even those of respiration, also become convulsed, whilst every muscle of the body is thrown into spasmodic action to resist our efforts,—the exciting agency of pain on the nervous system at large. What becomes of this vaunted muscular resistance in paralysis of a limb? Even in a low state of the nervous energy muscular power is reduced to a mere nonentity. I once attended a young man, much debilitated by nervous fever, who, rolling in bed, struck his shoulder against some lumpy place in the bolster. He complained so much that I was induced to examine the limb closely, and could scarcely believe it possible, yet found it to be true, that he had dislocated the head of the humerus, which was again almost as easily reduced from the feebleness of effort on the part of the muscles to resist us.

in a doze. He looked on me with seeming horror, fearing, no doubt, some new molestation. I spoke kindly to him, and did not touch his thigh, seeing it rested well upon the pillow, but more bent toward the pelvis. He laid a little on the affected side. At his usual time of rising in the morning he, for a moment, sat up in bed, and his mother was alarmed lest he should "put the bones out of place," or do some injury, but "finding," she said, "that he was not able to get out of bed, he fell back again, and had not moved or tried to get up since." I sent him half a grain of acetate of morphia, dissolved in ℥ij of oxymel; and water, ℥iss. Of this he took two teaspoonfuls at night, and lay composed until morning. No more spasm; never took another dose of the anodyne.

7. Could not visit him to-day. His father called to say his boy was easy and quiet.

8. The little boy looked on me with more confidence, and, with a smile at his mother, then at his pillowed limb, and then at me, seemed to ask why he was obliged to lie in such a novel position. The pillow had slid from under his natal tuberosity, and the edge of it rested just below it. The bandage was getting loose, the knee was more bent, with the heel drawn back towards the thigh, but the flexure of the thigh itself astonished me. The three ilio-psoal muscles, with the pectinalis, all seemed to be in contraction, and the thigh was so much bent upon the pelvis that my first impulse was to bring it down to the point of semiflexion; but a few moments consideration convinced me that so marked an action of those muscles, which had pulled the thigh forwards and upwards so much more decidedly in this case than I had ever before seen, must be the effect of nervous irritation. The blow having been received upon the fore and inner side of the thigh, and the branches of the anterior crural nerve struck, in order to prevent its branches being over-stretched, the limb was thus much fore-shortened. In this state of things, the muscles being no longer obedient to their owner's will, but placed for a time beyond his control, they had slowly, and almost imperceptibly, from the first day, contracted more and more, until they lay quiescent in the singular position in which I saw the limb. It was, then, to humour the organs of feeling, the nerves, that the limb was bent by the muscles, which acted in this case, as they always do, as the vassals, the most humble servants, of the aforesaid nerves.

Talk of humouring and bending the muscles! It is to ease the otherwise overstretched nerves that they bend: they are the servants, and not the masters—an effect, not a cause. I did not presume, then, to pull down the limb.* I dared not alter a position for which I had so much respect.

Let us, then, hear no more of the ignorant jargon about not humouring and bending the muscles. Propitiate the nerves, and fear not its muscles,—they are the friends of the fractured part, and a nidus for the nutrient vessels while they run to deposit the necessary osseous matter, which splint-apparatus and tight bandages obstruct, though they cannot altogether prevent.

To return to the patient. A basin of warm water and a sponge were procured, and the ends of the bandage turned off by the mother, and the thigh she well bathed in every part, as he would not allow *me* to touch him at first. The knee and ankle were resupported by doubled bits of cloth inserted between the pillow and the bandage, and the bandage, the pillow, &c., were restored to their situation. He took two doses of castor oil, and, by his own request, was taken down stairs to dinner, resting on the pillow, in two days before the end of three weeks. He walked slowly *in a mouth*, pointing his toe well, and no stranger to the accident could by the eye, or by examining his thigh with the hand, suppose that any fracture had ever happened.

What will the enemies of *position*, who are friends of *rigorous extension*, say to this case? Here was an unquiet boy, as turbulently-disposed a little fellow as could well be, completely subdued on the second day, and thenceforward lying quiescent,

* To illustrate what I mean. A medical man, by an explosion, suffered a transverse lacerated wound in the palm of his hand, which, wounding the branches of the median (or radial) nerve—the largest in the forearm—caused a powerful contraction of the flexor muscles, so that the hand remained closely shut, and any attempt, even the slightest touch, to open one finger, gave the instantaneous admonition of pain, by which the owner sensibly thought he was admonished to desist from the attempt. His medical *friend* thought differently, and his advice was, in his own conceit, wiser than Nature's monitions. He advised that a round piece of wood, like what sailors call a marlin-spike, should be slowly forced in from day to day, between the hand, thumb, and points of the fingers, until the parts were gradually forced open. The patient was here a better philosopher, and trusted to immersion in warm water night and morning, to anodyne liniments, instead of using violence to his oversensitive nerves; and it was curious to watch the process, the tardy process, of his hand's re-opening; at first a line or two a day; then an eighth of an inch, until, after a lapse of two months, restoration was complete, and, except some loss of the adductor pollicis, which was also torn through, the action of every finger is perfect as before. The one medical man was a physiologist; the other, who would have used force, was, so far, a mechanic only.

without constraint, and, after four or five days, easy and smiling, with his bed bestrewed with his toys, able to enjoy them, under as bad a case of simple fracture of the thigh as could well happen. Suppose he had been laid on any "apparatus," or had been bound in splints, however well padded, would he have laid so much at his ease, and could he have borne a state of straight extension at all? And if folly had prevailed so to place him, how great must have been his suffering!

Attention was challenged to this case in the first week of its progress, and many persons saw it, and were surprised to find that this little fellow would lie still, day and night, for a fortnight, without moving his thigh from the pillow, from whence he could (say the splinterers) have disengaged himself at any moment. No such thing; it is all a mistake! Pain, and the natural dread of it, were, as in Mr. Hele's little patient, a monitor (see next case but one), in the breast of a mere babe, to give it an instinctive idea for the necessity of its lying still to ensure its well doing; unconscious of the *why* and the *wherefore*, there was a necessity for its limbs now to lie still.

Nature, unmolested, is, in cases of fracture, ever true to herself, and, to prove that this may be received as an axiom, take another case.

Case.—The little daughter of James Coombe, labourer, of Kingsteignton, a child who was two years of age only on the 1st of March, 1836, on the 18th of the following May fell over the stairs, and fractured her thigh, at the central part of the femur. The parents being poor people, some difficulty arose how best to dispose of the little creature. The cradle was the only place of rest that could be had for her, and to bring it to a plane, the lower part was filled with straw, and on this and the bedding a long pillow was spread. She was laid on it, with her little fractured limb (very short in its dimensions) lying inclosed in it and the bandage. Slight spasms, in this case, continued to the third day, in which time she took two phials of anodyne, similar to the preceding, containing, in addition, a quarter of a grain of tartarised antimony, which kept up a little moisture of the skin, while it obviated costiveness. Things went on well until the fifth day, when I felt some perplexity, for the straw had sunk away unequally, and I feared lest the limb should, in some part, have deviated from the desired plane. By inserting a hand, slowly and cautiously, the pillow was raised up where it had sunk, and some flannel being thus introduced, the whole looked in better order; but I will confess that even twenty-two or twenty-three years of success still find me sometimes, as it now did, anxious for the result. Relying on the great general principle of an easy limb in a tolerable position, I felt again assured of ultimate success. At the end of a week the child had sunk much into the cradle, and particles of cake, bread, &c., had fallen about her, while the urine, in spite of every care, had flowed into her clothing, and excoriated the skin. All this made my little patient uncomfortable and her friends fearful, but I begged them not to disturb her, and on the tenth day a Mrs. Edwards kindly assisted in removing her clothing, and putting on what was clean. The excoriated skin was bathed with tepid water, and anointed, and the limb was washed all over more carefully than it had been, dried, rebandaged, and the child was laid in a bed.

June 3. The child does not now look afraid, and lies very much at ease, amusing herself; and her mother tells me that she is disposed to turn frequently in bed; so the pillow was untied, and the loops were removed, to allow of more freedom to the limb.

5. This day the little creature turned suddenly, and got on her knees, face, and hands, to her mother's surprise, and her fear too. She was gently replaced, and chid, to prevent a recurrence of the feat. My advice was to let her alone, saying that Nature would guide her actions, and while on the soft bed she would not hurt herself.

She recovered and walked, slowly at first, in less than a month, to the great joy of her parents, who, with no little pride, exhibited her unexpected performances, in this respect, to every comer.

This case shows the great superiority of freedom and absence of constraint over the coercive plan. The little creature, a mere infant, endured some smart of the cuticle, from excoriation, the consequence of lying in a pit, from which I was not hasty to remove her, that it might be shown that no danger was to be apprehended of her displacing the bones,—the greater injury of the two. To guarantee safety against this latter accident, the minor evil, the smarting, was permitted. In order to show the perfect safety of our system, and, in good faith to the principle, to perfectly adhere to it, a little indifference was shown to the minor cause of complaint, which I should not again perhaps allow to continue.

Among the communications which I have received is one from Mr. Hele, an able surgeon, of Ashburton, Devon, who thus writes:—

"My dear Sir,—Business has prevented me from sending before, the following case, which you are at liberty to use in any way you think best:—

"Susan Hynes' child (of this town), aged two years, in February last fell over the stairs, and fractured the femur. I saw her very quickly after the accident, and thought this a favourable opportunity to try the splintless method, particularly as I lived so

near the child that I could watch the case narrowly. She was put to bed, with her leg laid on a pillow; the fractured ends of the bone were placed in apposition, and the child soon became more easy and at rest. On the evening of the same day the parents complained that I had not used splints; I therefore applied a small splint to the side of the pillow, but so that it could not in the least interfere with the limb. No one unpleasant symptom occurred; she kept her leg and thigh quite still, and never in the least displaced the bone. In three weeks she was allowed to crawl about the bed, with a bandage around the parts; and from thence she began, by degrees, to walk, and she is now able to walk as well as most children of her age, while her limb is as straight as ever."

Mr. Hele, in conversation, further said, that to splinter up such a little creature was repugnant to his feelings; and he added some very kind expressions on the excellence of the plan that assisted in the cure without harshness or severity. He was pleased to say, that if the merit should be awarded me of successfully urging the banishment of splints, it would be one of the greatest achievements in surgery that has been obtained in modern days. Very many thanks to this kind-hearted surgeon (of a neighbouring town) for his assistance in the cause of science and humanity.

Mr. Ayliffe practises the various branches of our profession at Bovey Tracey, five miles from Newton. Mr. Ayliffe took the degree of B.A., at St. John's College, Cambridge. His letter, dated June, 1836, relates that, in the February of that year, "John Facey, aged 73, was injured by a fall; on examination a fracture of the neck of the bone, but external to the capsular ligament, was discovered. This case,"* he says, "as you well know, was treated without splints (which is now our confirmed practice), and this very old man in a few weeks was not only cured without deformity, but has been able to walk three miles at one time, with the aid of crummocks in his hands."*

Such cases as these are records of the triumphs of mercy over needless cruelty; and as I am not quite insensible of the offensive personalities—the charges of ignorance and folly—which have been unceremoniously directed against me and my mode of treatment, I hail these records as the best set-off against the opprobrious epithets, thanking such gentlemen as Mr. Hele and Mr. Ayliffe for their unbiassed opinions and support in practice. On the minds of the opponents of the plan I would impress the fact, that a proposal must be excellent indeed, in principle, when neighbouring surgeons coincide in its praises, and when such plans emanate from one among them whose business, of necessity, sometimes clashes with theirs.

I could enlarge this article to great length, by selecting other cases which have been sent to me, but as they describe simple fractures, without any particular feature of interest differing from what has already been given, I shall, without giving umbrage, wave them, and insert one more, that will interest Messrs. Bryant and Dendy, as being a reply in point with those gentlemen's share in the discussion at the London Medical Society, on Nov. 30, 1836. It is in respect for the opposition of Mr. Dendy, and with deference to his remarks, which were certainly very warm, that the following case is related, and because it is of a similar nature, and, in conjunction with Mr. Ayliffe's; and they will together, I hope, have some influence on his (Mr. Dendy's) future practice:—

Case.—Jane Carter, of Newton Abbott, was 72 years old in November, 1835. In the summer of that year she was seized with sanguineous apoplexy, was bled and purged, and recovered, with paralysis on the left side. Some weeks afterwards, being but partially restored to strength, when sitting unsupported in her chair, she fell on the concrete lime-ash floor, and fractured the right femur, just above the trochanter. Mr. Gage attended her, and, residing next door to her, I also was called in. The crepitus was evident, for it could be heard as well as felt, on motion. Mr. Gage humanely said, that to splint up this poor creature he thought would not be judicious; it was only for me to consent. Mr. Gage well knew that I should coincide with him in not splintering a fracture, and the conscious

* To elucidate this case and Mr. Ayliffe's remarks, it should be known that John Facey is a pauper, a class of persons who being sometimes less attended to than more affluent persons, are, with their friends, prone to construe what is not done (though rightly omitted) to the poverty of the patient. The poor people mistook the kindness of Mr. A. in not splintering his poor old patient, and referred the omission to any cause but the right one; and I see in my journal that, on the 11th of February, a few days after the accident, I was requested to visit this man, and give an opinion on the case, with Mr. Ayliffe. The friends at length assented to what we thought to be right treatment in such a case; therefore nothing was applied to his thigh but a soft plaster, an embrocation of a sedative, soothing nature, over the fore part of the thigh, and a hip bandage, I believe, when he was able to walk out.

* The crummock is a short walking-stick, on the flat top of which the hand presses.

feeling produced a mutual smile. So the motion for non-splinting was carried at once. She was much extenuated, and to defend the parts from pressure, knowing that she would have to lie on the affected side for a long period, and to restrain the parts gently in apposition, a large and soft plaster, spread with emplast. lithargyri, was applied over the site of the fracture, which partially embraced the limb. She suffered much pain, and to relieve it was supplied with half-grains of gum. opii, in pills, which were taken at night; and when such pills are recently made, with all due deference, I think they are better (for the patient, at least), than the pompous officinal preparation of opium, with fossil alkali and oil, and what else we do not exactly know. Besides these pills she took castor-oil occasionally, in the morning, or infus. sennæ, to obviate costiveness. At length her pains were moderate, and she could lay quiescent in bed, and passed the winter.

The idea of her walking again was scarcely entertained, but what was my surprise, one fine day, to overtake her, slowly pacing down the street, on the paralytic side supported by a female, and on the side of the fractured limb her hand leaned on the "crummock." I was curious to observe that her legs were of the same length as she stood upright on the ground, and her neat little foot was pointed as properly as the other. I have seen her walk more than once since. She is alive, and is now more than 73 years of age.

In Mr. Bryant's case the bone was split, therefore a restoration, perhaps, could not take place. Mr. Symes, a surgeon, of Edinburgh, refers to such cases in his synoptical work of two volumes, for students; and Mr. Dendy is assured that no constraint was imposed on either of the two last-mentioned cases,—no *pelvic straps*, nor restraint of any sort, more than that of a plaster, but each of these poor old folks indulged their aged limbs upon a soft feather bed, at their humble abodes, and there slowly recovered—perfectly recovered; but a very different result would most certainly have followed, if the feather bed had been supplanted by the hard mattrass of a hospital.

In concluding, I must take the opportunity of thanking Mr. Crisp for his recommendation at the Medical Society before alluded to, "that Mr. Radley's plan should be tried before decided on; for he had seen cases in the hospital that would have done better without splints than with them." How opposite this remark to that of the famous hospital surgeon, who philosophically said he *thought* that crooked legs would follow the non-employment of splints, and therefore would not make the experiment!

Finally, I beg to say, that any opinion on the subject, which is given without the previous employment of full, fair, and rigid tests, and subjected to proof in practice, are mere postulations, airy hypotheses, which are unworthy of adoption after the evidence which has now been laid before the profession on behalf of the new mode of treatment.

IMPROVED MODE OF REDUCING ARSENIOUS ACID.

To the Editor of THE LANCET.

SIR:—I beg to submit to your notice the following observation on the reduction of arsenious acid by the flame of hydrogen gas, which appears to me to be a useful improvement on the method invented by Mr. Marsh. In the experiments which have hitherto been conducted, the gas has been allowed to escape upwards, by means of a straight tube, and the crust of metallic arsenic has been deposited upon a plate of glass, held immediately over the flame. By this process the arsenuretted hydrogen is rapidly dispersed, carrying with it a great deal of the metallic arsenic unreduced; consequently, a small portion only is collected on the plate of glass; but if the plan which I have adopted be resorted to, a much greater quantity of the gas may be collected, and the decomposition is rendered more obvious.

This method consists simply in the employment of a tube, bent twice, at right angles, of which the extremity is to be firmly luted into the bottle containing the materials for hydrogen gas and the solution of arsenious acid. When the gas is passing over briskly, a light is to be applied to the free extremity. In a few minutes that extremity will be completely coated with the metal. In this manner a tube, of the diameter of an ordinary quill, may be encrusted, to the extent of a $\frac{1}{2}$ of an inch, by five drops of the cold boiled solution of arsenious acid. The advantage of this method is obviously to preserve as much as possible of the metal, a great part of which is dissipated with the gas when the straight tube is employed. In the curved tube, on the contrary, the gas will continue to burn with a slow lambent flame for a long period, and gradually deposit the metal upon its inner and outer surfaces. The vitreous appearance of the free extremity is completely destroyed, and that portion of the tube resembles a pewter or leaden pipe. I have the honour to be, Sir, your obedient servant,

G. A. F. WILKS, M.D.,
Lecturer on Materia Medica and Botany in the North Lon. School of Med., &c.

5, Hart-street, Bloomsbury,
Nov. 27, 1838.

ON LARYNGITIS AND ACUTE DISEASES OF THE MUCOUS MEMBRANE.

By D. Lewis, *Esq., Surgeon.*

Having experienced, in many cases, the good effects of a combination of calomel and tartarised antimony, in acute diseases of the mucous membranes, without the aid of topical or general bleeding, I am now disposed to consider the abstraction of blood not only useless, but absolutely hurtful. The primary cause is an obstruction of the natural secretion, occasioned by cold, or some other agent, which induces a morbid action in the part; it is, then, evident that the natural secretion of the membrane must be first restored by medicines acting specifically on the disease, and not by lowering or depressing the system by the abstraction of blood; for, unless the diseased action be first put a stop to, the inordinate action of the blood-vessels, consequent upon the abnormal condition of the parts affected, cannot be restored to its regular and equable motion; but as soon as the natural and healthy secretion of the membrane is restored, the superincumbent layer of exuded lymph is detached, and thrown up in the shape of false membrane, or loosened in such a manner as to be expectorated without difficulty, and then the increased action of the circulating system will subside of itself; sublatâ causâ, tollitur effectus. It may be questioned whether the theory of inflammation, as taught in the schools, has not retarded rather than accelerated the progress of medical science; the origin of almost every disease is attributed to inflammation (which, in many instances, is only the result of disease), and the practice has been to abstract blood, without taking into consideration that, by depressing the vis vitæ, they only assist in overpowering nature. Abstraction of blood may be proper, in a limited degree, in affections of the serous membranes, although I have found in my own experience, a combination of hydrarg. c. cretâ and James's powder answer every purpose in effecting a cure in most cases. Experience in medicine is like a staff in the hands of a blind man,—if he does walk but slowly, he goes safely, but if he neglects the use of it, in order to walk faster, he is sure to fall. The following case of laryngitis and inflammation of the trachea, out of many I have seen, is meant to prove the reliance we may place in the combination of calomel and tartarised antimony in acute diseases of the mucous membrane.

Mrs. D., ætat. 37, in the 7th month of pregnancy, had occasionally experienced, for a fortnight previous to an attack of acute laryngitis, a sensation of dryness and tickling at the posterior part of the throat. On Saturday, the 27th of October, after having been exposed more than usual to the alternations of heat and cold, she was seized, towards bed-time, with a choking sensation in the throat, which opposed her efforts to cough; her voice was gone; she could hardly swallow; was unable to lie down, but was compelled to be constantly struggling and fighting for breath. On the afternoon of Sunday I saw her; she was then flushed in her face, labouring hard to take in the air, restless, and anxious; pulse quick; she appeared to me then suffering under such a formidable disease that, unless the morbid action that was going on in the trachea and larynx was speedily arrested, she could not survive twelve hours. Under these circumstances, I was anxious to adopt the most efficient treatment, without doing too much, which was as follows:—*Calomel*, gr. i.; *antim. tart.*, gr. ¼, every two hours, and a blister over the throat. On Monday the breathing was greatly relieved; the patient had taken twelve of the powders; continued taking them during the day. On Tuesday the same medicines, with a blister from the superior part of the throat, extending along the sternum as far as the ensiform cartilage, merely to excite counter-irritation. On Wednesday morning, after having taken thirty grains of calomel, and seven grains of tartarised antimony, without any evident effect on the secretions, she felt sick, and vomited up a COMPLETE TUBE, OF FOUR INCHES IN LENGTH, OF FALSE MEMBRANE, which was as solid and as well organised as the mucous membrane of the trachea; from this time the patient experienced no difficulty of breathing; the calomel and antimony were discontinued, yet detached pieces of membrane were expectorated for a few days afterwards; but a healthy secretion took place, and the lady is now well, with the exception of her voice, which probably will not return until after her confinement. The false membrane that was thrown up is kept in spirits, and may be seen by any one desirous of viewing it.

28, Artillery-place West, Finsbury,
Dec. 1, 1838.

Morphine.—The following statement has been made by M. Raspail in his "System of Organic Chemistry":—"1285.—But morphine acts much less energetically than opium; one or two grains of the latter are sufficient to produce sleep, and a few grains more will cause death; but, according to recent experiments, 29.5, and even 59 grains of the acetate of morphia, which is its most active combination, do not produce death, whether taken internally or injected into a vein."

DISCREPANCIES OF OPINION ON THE RESPIRATION OF CARBONIC ACID GAS.

NEW VIEWS OF THE VAPOUR OF BURNING CHARCOAL.

To the Editor of THE LANCET.

SIR:—Having read in THE LANCET for the present week some explanatory remarks by Dr. Golding Bird, upon his reported evidence on the inquest of Trickey, who was found dead in St. Michael's Church, *in company with* one of Messrs. Harper and Joyce's stoves, I am induced to intrude a few more lines upon this very important subject.

Dr. Bird has stated the received opinions of physiologists to be, that an atmosphere containing from 8 *to* 10 *per cent.* of carbonic acid gas, will, if respired for a long time, produce coma, and subsequently death, from spasmodic closure of the glottis.

Dr. Ure, in the third number of his "Dictionary of Arts, Manufactures, and Mines," published on the 1st of November last, has stated, under the article "Carbonic Acid," that if air contain more than 15 *per cent.* of this gas, it will be unfit both for respiration, and for the support of flame.

Dr. Turner, in his "Elements of Chemistry," informs us that a taper will not burn in a mixture of four parts of atmospheric air, and one part of carbonic acid gas; *i. e.*, when the air contains 20 *per cent.* of carbonic acid. We are also told that human beings cannot live in such an atmosphere.

Dr. Christison has retrograded a step, and quoted an exception to the very generally received opinion that *a man may venture into an atmosphere contaminated by carbonic acid gas, provided the carbonic acid do not exceed the limit necessary for the combustion of a candle.*

I cite a case, which occurred in my own neighbourhood, of a man descending into an abandoned coal-pit, in which ignited candles were repeatedly extinguished, and remaining in it for several minutes, until he successfully effected the object of his descent, viz., bringing to light the body of an unfortunate child, who had, through its youthful wantonness, been precipitated from a part of the old machinery.

Now all these different statements tend to bring each individually into discredit; and if these many discrepancies on this one subject are not sufficient, we have the opinion of Dr. Christison, that carbonic acid acts *as a narcotic poison*; and the subsequent assertion of Dr. Thomson, of Glasgow, that carbonic acid gas *is not poisonous*, but that it causes death by suffocation, in the same manner as though the party were submersed under water.—(*Vide* Dr. Thomson's evidence on the trial of Mr. Muspratt, of Liverpool, for an alleged nuisance.)

Dr. Christison may, probably, have attributed the deleterious effects of the combustion of charcoal (as most other persons have done) to the carbonic acid evolved. I feel perfectly convinced that this common supposition is erroneous, and that the combustion of charcoal is attended by the evolution of some peculiar mephitic effluvia which have never yet been examined; and that, whether the charcoal be of the ordinary description, or whether it be "well burnt," *by the continued application of a white heat for forty-eight hours*, the same injurious vapours will be given off. It is to this point that I should wish to direct the attention of Dr. Bird, whose natural acumen and cultivated talents are well suited for such a task. If charcoal be ever so well burnt and dried, it will, if ignited immediately after it is cold, give off much aqueous vapour, and some peculiar matter not visible to the eye unless it be condensed upon an interposing surface. This matter, when sublimed upon an iron plate, has the appearance of the bloom upon white grapes, and resembles it in colour. If a little of it be scraped from the plate, and heated in a glass tube, it will be found to be a hydrate. Moisture will appear at first, having *an acid reaction*; then a ring of white matter, resembling the volatilisation of arsenic, and, by continuing the heat, we get a second ring, very much like the last in appearance, but not ascending so high. What these substances are, remains to be proved. I detected them this very morning, and shall pursue the investigation.

When I reported to you the result of my own nocturnal experiment with the stove, which you kindly published in one of your November numbers, I said that I felt almost recovered. It was fully a week before the effects vanished; and, during the period of recovery, the smell of a candle that required snuffing, or of an extinguished lamp, was acutely annoying to me. I should also have told you that, being accustomed to endure all the sulphureous and carbonic exhalations of an extensive glass manufactory, and also of vitriol, sulphate of soda, and alkali works, I am not so susceptible of annoyance from gaseous emanations as many other folks. A lady, who was on a visit at my residence, thought it fit one day to include the very stove which I have already described in her dressing-room, whilst she was preparing herself to dine. She had been about half an hour in this little apartment, which contained about 800 cubic feet of air, when she was seized with vertigo and faintness, and rendered perfectly unfit to make her appearance down stairs during the whole evening.

In this case, as in my own, the carbonic acid could have amounted to so small an excess above the ordinary atmospheric standard that it would be superlatively ridiculous to ascribe the consequences to the respiration of carbonic acid. Begging pardon for being so tedious, believe me, very truly yours,

CHARLES THORNTON COATHUPE.

Wroxall, near Bristol, Dec. 1, 1838.

THE LANCET.

London, Saturday, December 8, 1838.

To what general system of treatment are the 28,000 English lunatics now exposed? What remedial agents are resorted to? What hygienic means? What moral discipline? We have given a striking example of the *moral treatment* to which lunatics are exposed in some respectable English houses, and at another time we shall adduce others, not less striking, from the Parliamentary Evidence taken by the last Committee of Inquiry. A good illustration of the medical treatment of pauper lunatics appeared in the newspapers not many days since. It was elicited, in a public inquiry, that the lunatics of the Kensington Union had been shut up in a crowded room, rarely seen by the surgeon (Mr. WILSON), and not allowed even aperient medicines to regulate the bowels. Some of the unfortunate patients remained a week without any alvine evacuations; while it is admitted by all writers upon insanity, and established by reason, common sense, and the experience of centuries, that the regulation of the excretions is of vital importance. ESQUIROL expresses the general opinion when he says, that the secretions and excretions should be favoured in every possible way; that care must be taken to see that the bowels act, for *constipation is a frequent symptom, and exasperates the delirium.** But Mr. WILSON, it is said, explained to the Guardians that his salary was inadequate, and neither enabled him to devote time nor physic to the destitute pauper lunatics. The discovery of the wretched condition of the lunatics had not been made by the *Guardians* in their official capacity; it had been forced upon them by an accidental visitor, and, instead of remedying the evil, the Board applied themselves forthwith to obviate the possibility of any future discovery of the same ungrateful description. It was resolved, that no one should see the lunatics without an order from the Chairman. Other persons would have sought to remedy the system—to have provided against the recurrence of any neglect—and, in other countries, the authorities would, probably, have taken measures to demonstrate the improvement, and to propitiate public opinion. It was reserved for Englishmen—no—for the Guardians of the Kensington Union, to trample upon the defenceless lunatic; to shroud their deeds in darkness, and to brave public indignation! What do the *Guardians* propose to do with their *secluded* lunatics? Do they desire still further to *exasperate the delirium?* Is this their tyrannical pleasure? Would it not be more humane to restore their unhappy brethren to reason than to consign them to the arms of death, or to perpetual madness? Would it not be more *economical?* Yes, would it not be more *economical?* We reiterate this question, for we are addressing the *Guardians of the Poor.*

From the letters which were formerly addressed to us by certain influential Guardians in the Kensington Union, we should fain hope that the Board would yet vindicate its claim to human feelings, and rescind the order, which can only be considered as expressive of their determination to deal cruelly with their lunatics under the cover of secrecy. Why, we ask, stain the national character? Why hold yourselves up to universal detestation?

This instance is important, as vast numbers of the insane population are in the hands of the Guardians; many are confined in workhouses; and more are every day drafted into the new establishments, to save the expense incurred at private and county asylums.

It is admitted on all hands, that the treatment of insanity should vary with its nature, forms, causes, stages, and complications. No specific remedy exists; but practitioners generally agree that, in acute insanity, antiphlogistic measures should be

* Vol. I. p. 142.

adopted, and varied according to circumstances. Respecting the general principles of treatment, in the chronic stages, there is less substantial agreement; an endless variety of expedients have been resorted to, but with what relative success it will be impossible to determine until the results of all the cases are recorded. The warm bath, the cold bath, ice to the head, and the bath of surprise, are employed. VAN HELMONT insisted that the lunatic should be kept under water till he lost his senses; and VAN SWIETEN advocated this barbarous practice, which, with bloodletting, was the principal means used in the last century. Drowning, and bleeding to death, chains and starvation, were the sovereign remedies. To drain away the mad blood was a capital indication. The patients of large asylums were bled in spring and autumn, and then thrown, bound hand and foot, into an adjacent river, or pond. When recovery occurred under this system it was ascribed to the treatment, and very justly looked upon as a miracle. PINEL denounced the irrational practice in France, yet ESQUIROL attended a lunatic who had been bled thirteen times in forty-eight hours. AVENBRUGGER recommended copious libations of water, and LEROI D'ANVERS, THEDEN, and HUFELAND, speak favourably of cold water where there is a disposition to suicide. As we have already stated, there is no difference upon the point that the bowels should be duly regulated; but the ancients administered drastic purgatives, such as hellebore, in melancholy. COX gives to emetics the first place in the cure of insanity. Tonics have their advocates. The utility of setons, moxa, the cautery, and blisters, is generally admitted; but the list of remedies is endless, and the most conflicting opinions with regard to their efficacy prevail. The Metropolitan Commissioners in Lunacy might have thrown invaluable light upon the influence of the methods of cure in use in the English asylums, if they had sufficient knowledge to render the Commission useful. The proportion of recoveries at the public asylums is often very different, and this coincides with a difference in the treatment. At the Gloucester Asylum, for instance, Mr. HITCH states, "That the use of the "lancet, leeches, cupping-glasses, blisters, "drastic purgatives, the practice of shaving "the head, are totally proscribed." The patient's bowels are kept open, his general health is attended to, and stimulants are given; a generous and nutritious diet,—bread, meat, ale, &c.—are allowed. Living in the open air is enjoined; the douche and bath of surprise are used.* Here is a plan of treatment differing very materially from that pursued in other asylums, and what was the recorded result? Of 446 admitted, 224 recovered, 55 died; in other terms, 50 per cent. *recovered*, and 12 per cent. *died*. At the Hanwell Asylum (1831-5) only 19 per cent. recovered, while 27.5 per cent. died. The mean duration of the treatment at Gloucester was 1¾ years; at Hanwell, 4 years. What was the cause of this enormous disparity? Was it not the system of treatment? All the details should be carefully investigated. No one will deny the importance of ascertaining whether remedial measures can produce a difference of 190 and 502 recoveries out of 1000 cases treated. The restoration of ten persons—aye, of one person—to reason, is of the utmost moment. For the sake of the Guardians of the Kensington Union, let us merely calculate the *pecuniary* advantages of successful treatment. At the Stafford Lunatic Asylum (1831-5), 46 per cent. of the lunatics recovered at the end of 1½ year; while at Hanwell the patients were kept more than four years, and in that time 19 in 100 recovered. The treatment of a patient at Hanwell would cost the London parishes 70*l.*, at 6*s.* 7*d.* per week; at Stafford Asylum, 19*l.*, at 5*s.* a week. The *cure* of a patient at Stafford would amount to 41*l.*; of a patient at Hanwell, to 373*l.*; because, after *five patients* had been treated for four years at Hanwell, only *one* would recover. The

* Prichard on Insanity, p. 261 and 276.

diet of the insane is of great importance. It differs to an amazing extent in different asylums, and no steps have been taken by the Commissioners to ascertain, directly, the effect of high and of low diet upon the course of the malady. In the county asylums the greatest discrepancies prevail. Meat is allowed at all of them. At the Gloucester, Kent, Nottingham, and Stafford asylums, the patients have nearly 3lbs. (2.87) of solid cooked meat, and cheese, every week. At York, Suffolk, and Hanwell, little more than 1lb. The weekly allowance of bread varies from 5lbs. to 10lbs.; puddings, flour, &c., to the amount of 1 to 4lbs. are added to this item. Potatoes are the principal vegetables. The weekly quantity of solid food, exclusive of vegetables, was 10 to 12lbs., and about 20 per cent. was animal matter. Fourteen pints of beer are allowed weekly at the Gloucester, Kent, Nottingham, and Stafford Asylums; no beer is allowed at the York or the Middlesex County Asylums. The dietary of the keepers is only mentioned at Lancaster and Kent. Strength is required in these persons; and a very firm conviction appears to have been entertained that it depended, in a great degree, upon the amount of food, particularly animal food.

Keepers at Lancaster Asylum, weekly,—meat, &c., 6.5lbs.; bread, &c., 13.5lbs.; potatoes, 14lbs.; solid food, 34.0; proportion, 1.00.

Patients at Lancaster Asylum, weekly,—meat, &c., 1.5lbs.; bread, &c., 7.1lbs.; potatoes, 12lbs.; solid food, 20.6.; proportion, 0.61.

If the keepers are on full diet, the patients are on half diet. These facts have been deduced from a Parliamentary Return*. It would be in vain to look for any useful information from the present Visitors and Commissioners; they do not inquire into the treatment; they neglect the dietaries; they look to none of the great surrounding agencies which will restore the maniac to reason, or plunge his soul in perpetual madness. They *now* make no report; they merely perambulate the asylums at their convenience, and draw their salaries. How long is this system to continue?

Since the above article was written we have perused a long paper on the "*Treatment of Insanity in England*," which has just been published in the December number of the *Monthly Chronicle*. (Longman and Co.) Towards the conclusion of the Essay there are to be found many valuable suggestions, well deserving the attention of the Metropolitan Commissioners and others who may feel any interest in proposals that are made for the purpose of rescuing from disease and wretchedness the most afflicted portion of the human race.

* Statistics of English Lunatic Asylums, p. 16—18.

THE VIRTUES OF ANIMAL MAGNETISM.

Long before the days of Mesmer and his deluded followers, the effects of certain manipulations, now called "passes," on the frames of delicate or nervous females, were known to libertines. That such effects may be readily produced, at the present time, is familiar to every physiologist, and to many of the "men about town." The latter, however, influenced by a salutary fear of the law, avoid practising in public their mesmeric tricks, lest some common-sense view of the case might construe them into what they really are, *indecent assaults;* and, on the other hand, we venture to affirm that there is not a single physiologist in the kingdom, possessed of moral feeling, who would excite, day after day, the nervous systems of young and artless females by a series of manipulations which not only injure the body but frequently lead to a loss of virtue.

Many examples of the latter melancholy effect of mesmerism might be adduced. One will suffice; we relate it on the authority of the President of the French Institute. A young French lady, the daughter of a

wealthy banker, was placed under the care of a Mesmeriser. The follower of MESMER was a Polish quack, professing to be a physician. We know not, of course, what may have passed between the impostor and his patient; sufficeit to say, that, by some species of "dislocation,"—or other "anomalous transposition of the senses" which Mr. HERBERT MAYO alone could explain,—the young lady was thrown into a profound sleep, and the quack stole her honour. On the discovery of his infamy the Mesmeriser fled to *London*.

British Annual and Epitome of the Progress of Science, for 1839. Edited by R. D. THOMSON, M.D. Bailliere, London.

WE last week noticed, as a useful contribution to medical literature, the "British Medical Almanack" of Mr. Farr. The work now before us cannot fail to prove equally useful to the cultivators of science in general. It contains a variety of information of which we find ourselves in daily need, and several original articles of great interest. Thus, the tables of the different coins current of various kingdoms, together with their relative values; the measures of length employed in different countries; the medicinal weights of Europe and North America, reduced to English equivalents, &c., are highly useful, and seem to have been compiled with much care. The only omission which we perceive in this part of the work, is the absence of a comparative table of the different thermometers used in Europe. A table of this kind would have enabled Dr. Hammick to have avoided some of the errors relative to temperature into which he has fallen in his translation of Mitscherlich's Chemistry.

The statistical account of the British Universities will be read with much interest. Although necessarily imperfect (for the editor informs us, that applications for information, addressed to the Universities of this country, were generally treated with the most marked neglect), it affords matter for much reflection, particularly when we learn that the revenues of the Oxford, Cambridge, and Dublin Universities exceed, by no less a sum than 306,886*l*. the whole sum expended in France for Universities, schools, and every branch of education; although, in that country, the expenses of University and Collegiate instruction are exclusively borne by the Government.

Amongst the papers on general science we would notice, in particular, a very interesting memoir on the progress of steam navigation, by Mr. Russel, in which the claims of England to the discovery of "the steam-boat" are most distinctly established; and a learned essay on the characters and distribution of extinct animals, by Professor Grant. The editor has furnished a notice of the new chemical substances discovered during the past year. On the whole, we can recommend to our readers the "British Annual" as a worthy companion to the "British Medical Almanack," and this is one of the highest encomiums which we could pass upon it.

MEDICAL SOCIETY OF LONDON.

Monday, November 26, 1838.

Mr. BRYANT, President.

HYDROPHOBIA; REMEDIES FOR THE DISEASE.—CASES OF OBSTINATE VOMITING.

MR. HUTCHINSON related a case of hydrophobia occurring in a young woman, aged 26, who was in the fifth month of her pregnancy. He was called to her on Thursday, the 22d of Nov., and found her complaining of pain in the head and loins, with thirst, great langour, and depression of spirits; pulse 120; tongue furred; breathing laborious and oppressed, and her manner agitated. She had been previously in good health, until two nights before, when she got her feet wet, and shivered, and had not been able to sleep since. She was bled, a blister applied to the chest, and some diaphoretic medicine administered. There was, at this time, no suspicion of hydrophobia; but on the following morning the slightest motion of the air around, or the sight, or mere mention of fluid, produced violent convulsive respiration. There was great tremor of the limbs, and she complained that the urine was forced from her every time the dyspnœa came on. The bleeding had relieved her; her thirst was not so great; pulse 140; bowels costive. He was now informed that a terrier dog, which had been staying at the house, and whose manner, which was usually exceedingly mild, had become irritable whilst she was caressing him, had leapt up, and struck his nose against her under lip with so much violence as to loosen one of her teeth. There did not, however, appear the slightest abrasion of the skin; the dog ran away, but was killed a fortnight afterwards in a rabid state. The case went on during the day

without any alleviation of the dread of fluids, though some leeches, which were applied to the throat, afforded her relief. With great determination she was enabled, after several vain attempts, to swallow some toast-water out of a pewter pot, but she looked pale, wild, and haggard, and complained of great thirst. Her mind throughout had been perfectly calm and collected, and her spirits were good. About twelve o'clock she became insensible to external objects; her eyes were fixed; the respiration returned to its natural state; she gradually sunk until twenty minutes to ten o'clock, when she expired.

Sectio Cadaveris, 26 hours after Death.

General aspect of the body healthy, and that of a well-formed person; limbs moderately stiffened; no discolouration below the chin, except on the back, from gravitation, owing to the position in which she was laid at the period of death and afterwards; the face presented a brownish livid appearance, in the most intense degree, in the following order:—1st. The upper lip most livid, decreasing in intensity from its free margin; 2. The lower lip, thence diffused over the chin; and 3. The alæ of the nose, spreading off to the cheeks. On the most careful investigation of the skin of the lip, where the nose of the dog was described to have struck, there was no mark of cicatrix or abrasion whatever; but, on the mucous membrane of the upper lip, in apposition with the canine tooth of the right side, there appeared to have been an abrasion of the surface, leaving a small, circular cicatrix, having a central reddened point, whether occasioned by the force with which the dog struck the woman seems dubious, as the husband states it was the lower and not the upper lip which was struck. The parts examined were the tongue, the fauces, the epiglottis, trachea, œsophagus, lungs, heart, liver, and uterus. The apex of the tongue showed a small livid spot; the rest of the tongue presented the natural hue, but seemed increased in bulk at the base; no vesicles were discovered; the mucous follicles appeared large, and the vessels, especially the veins, were copiously injected with dark fluid blood; the fauces presented a similar appearance; the epiglottis was of a vermillion hue, which was not removed by washing or by pressure with the scalpel; the trachea was reddened and injected; the œsophagus exhibited nothing unusual; the lungs were largely injected, livid, bleeding freely on incision, the blood very dark coloured and fluid, the right pleural surfaces presenting extensive bridle adhesions; the veins on the surface of the heart injected, and the ventricular cavities both charged with dark blood, having an asphyxiated appearance,—that of the right being fluid, the left containing a piece of coagulum in the centre of fluid blood; the auricles nearly empty, probably evacuated from position. The stomach contained half a pint of a dark chocolate coloured liquid; its mucous membrane appeared here and there stellated and dotted, and permeated with large veins, intensely injected with dark fluid blood. The minute ramifications of these veins seemed so much gorged that their contents had transuded and become mixed with the contents of the stomach, giving the brown colour to the liquid ejected. The blood in the veins could be made to move easily forward under the pressure of the scalpel. The liver of a darker hue than natural, and when cut into bled very freely at each incision; the blood dark and fluid. The gall-bladder was distended with bile; the uterus contained a male fœtus, apparently about five months advanced, the surface of which was of an universal pinkish-brown colour, indicating a general venous circulation.

Mr. Dendy thought the case interesting, as occurring, probably, from the fright of the patient, no abrasion having taken place. It illustrated the fact, too, that inflammation of the fauces was not necessarily present in hydrophobia. Should he ever have a case of hydrophobia again under his care he should administer turpentine. In the case of a boy affected with this disease turpentine was administered, *ad libitum*, with castor oil. The symptoms were entirely relieved, but the patient afterwards died suddenly, probably from apoplexy.

Mr. Cole said abrasion was not necessary for the inoculation to take place. Dr. Hamilton had related a case where a lady contracted hydrophobia by merely passing through her mouth a skein of silk that had been played with by a rabid spaniel.

Mr. Field said cases had been related in which morphia, sprinkled over a blistered surface, had relieved the pain attendant on hydrophobia. Inflammation of the epiglottis bore many marked resemblances to hydrophobia.

Mr. Hutchinson inquired how he should act in the case of a little girl who had been bitten the same day by the dog that had attacked his other patient. The bite had been cauterised: no cicatrix was observable.

Mr. Crisp believed that Mr. Eagle was sanguine in thinking he could prevent hydrophobia by keeping up a mercurial action for some time in the system, and exposing the patient to a great increase of natural warmth.

Mr. Dendy recommended Mr. Hutchinson to pursue a course of prophylactic treatment with his patient, by remedies tending to keep down inflammation—purgatives and warm sea-bathing.

Dr. Clutterbuck thought, that as all the remedies which had been employed hitherto in hydrophobia had failed, every powerful article in the materia medica should be by

turns resorted to in the various cases, until some one of them was found beneficial. He should be inclined to employ bleeding, because he thought, that though this remedy had been resorted to in many fatal cases, it had always been conjoined with medicines likely to counteract its effects.

Some conversation followed on the efficacy of excision and cauterisation of bites, in preventing the occurrence of hydrophobia, some members considering these means of avail, and others relating instances in which they had failed.

Monday, December 3rd.

The discussion on hydrophobia was renewed.

Dr. J. Johnson believed that hydrophobia, like small-pox, being a specific morbid poison, would run its course uncontrolled by any internal remedy. Small-pox, indeed, might be modified by treatment, but it would go through its natural phases.

Mr. Dendy considered that there was an antagonising power in vaccination against small-pox, even when the vaccine lymph was inserted after there was every reason to believe that small-pox had been contracted, the disease produced by this proceeding being remarkably modified.

Mr. Field thought specific diseases would run their course unchecked by remedies.

Mr. Leese inquired what was the composition of the Ormskirk medicine, long celebrated as a preventive of hydrophobia? He understood it consisted, chiefly, of the leaves of box.

Mr. Cole knew a gentleman who kept fox hounds, who always gave them large quantities of box when hydrophobia was coming on, the first symptom of which consisted in the affected dog drinking the urine of others.

Mr. Linacre said that a gentleman had purchased a remedy, much in repute in one of the provinces, as a cure for hydrophobia. The chief ingredients of this remedy were box, bear's-foot, and primrose-root. The evidence on which the reputation of this remedy rested was equivocal. The recorded cases of hydrophobia, said to be cured by it, were too loosely drawn up to be accredited. He (Mr. L.) had not yet had an opportunity of trying its effects.

Mr. Crisp referred to a lecture in The Lancet, by M. Magendie, in which that physiologist injected water into the veins of a hydrophobic patient, who lived five or six days without pain, but suffered from general articular dropsy.

Dr. Johnson said the case which Magendie had related, he believed, was one of spontaneous hydrophobia.

A few observations followed, but the subject was generally considered as quite worn out.

Dr. Johnson said, that for the last year or two he had seen a great number of cases, occurring chiefly in females from 17 to 24 years of age, some of whom were chlorotic, some hysterical, some not so, in whom there was most obstinate rejection of all food from the stomach. In none of these cases was there the slightest evidence of any organic disease of the stomach. One of the most remarkable of these cases was that of a young lady, aged 19, who had not been able to retain any solid food on her stomach for the space of twelvemonths. She was much emaciated; no sign of any organic affection was discoverable. Pain, of an acute character, came on about fifteen minutes after taking food, and was not relieved until the food was rejected. The usual remedies for vomiting were administered without relief. It was then determined to give the stomach entire rest. Eight ounces of rich beef tea, with 15 drops of laudanum, were injected into the bowels, three times in the twenty-four hours. Nothing was given by the mouth, except a little milk or water to quench thirst. She slept an hour after each injection. She rapidly improved under the treatment; the stomach gradually regained its function, and she is now quite well. Other cases had occurred in which this plan was successful. He felt convinced that persons might be kept alive for months by the use of nutritive injections. Forty years since he had kept a man alive six or seven weeks, by injections of milk, and the use of milk baths.

Mr. Leese had found severe neuralgic pain of one side of the face at present very common. Arsenic he had found to relieve one case in which quinine was of no avail.

Mr. Dendy thought the changeable state of the weather might lead us to expect the occurrence of many nervous affections. He had now a case of such exquisite tenderness of the surface, in an hysterical female, that a feather falling on the abdomen produced torturing pain. He had also a case of complete aphonia, in a lady who had suffered for four or five days before from catarrhal symptoms.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, Nov. 27, 1838.

Dr. Bright, President.

MEMOIR ON TUPHLO-ENTERITIS, OR INFLAMMATION AND PERFORATIVE ULCERATION OF THE CŒCUM AND APPENDIX VERMIFORMIS.

By John Burne, *M.D., Physician to the Westminster Hospital.*

The author of this paper laid a memoir on the same subject before the Society in May, 1836 (vide vol. xx. "Med. Chir. Trans."), to which latter the present might be considered a supplement. The author commenced his second memoir by some ob-

servations, anatomical and physiological, on the causes and the comparatively pretty frequent occurrence of tuphlo-enteritis. He was of opinion that the ordinary result of spontaneous inflammation of the cœcum was resolution, but that perforation of the intestine was a consequence to be looked for in most cases in which the inflammation depended on an irritating substance impacted in the appendix. The cure of tuphlo-enteritis, by resolution, was then illustrated by particulars of three cases, the author insisting strongly on the use of mercurials and saline aperients, as the best remedies, in addition to local bloodletting. The history of perforative disease of the appendix especially, was then entered on. The author first pointed out the great variety of local relation for which the appendix was remarkable, the organ varying much in its place of origin, in its length, in its position, and presenting, in consequence, much embarrassing variety in its apparent seat and symptoms. Four cases were then given of tuphlo-enteritis, arising from perforation of the appendix, of which three were verified by dissection. The author then made some remarks on the diagnosis between spontaneous tuphlo-enteritis and that occasioned by disease in the appendix, in which he held that, in the former, when the bowels were relieved, amendment usually followed; whereas, in the tuphlo-enteritis, from diseased appendix vermiformis, no such benefit could be expected to follow purgation without the removal of the irritating substance. After, then, detailing a case of tuphlo-enteritis of the latter kind, the author made some observations on the literary history of the disease. The author, in the first place, stated, that he had in his former paper omitted, inadvertently, to make due acknowledgments to Dr. Copland, as an author, who had, in his Dictionary, given "extensive information on the diseases of the cœcum," and then proceeded to criticise the statements and opinions of several other previous writers, foreign and domestic, with a view to show that they had misapprehended the facts they had observed; viz., Dupuytren, Husson, Meniere, Dance, Louyer, Pouceau, and Villernay, in France, and Mr. Ferrall, in Ireland. The author then stated, summarily, his own views as to the causation of tuphlo-enteritis, viz.:—1. Local accumulations in the cœcum. 2. Presence of worms, concretions, &c. 3. Previous chronic disease of the cœcum. 4. Perforation of the cœcum or appendix. The author then made further observations on the diagnosis of tuphlo-enteritis in general, and on its varieties in particular, and concluded his paper by a numerical analysis of twenty-one cases of tuphlo-enteritis that had fallen under his own observation. Of these eight died; nineteen were acute; eleven were simple inflammation of the cœcum, and all recovered. Two were chronic disease of the cœcum, and both fatal. Six were instances of perforation of the appendix, of which five were fatal. One was perforation of the cœcum from within, and recovered; one was inflammation of the appendix, with peritonitis, and was fatal. Sixteen were males; two were under ten, and three over fifty, and the rest distributed over the intervening years. Six were gentlemen, the others of the labouring class; most occurred in autumn and the beginning of winter.

Dr. BRIGHT said, the disease described in the paper was by no means an uncommon one. Within the last month he had seen two cases in which it occurred; in one of these patients, who died of inflammation and abscess of the liver, disease of the cœcum was not even suspected. On examination an abscess was found, arising from perforation of the appendix vermiformis, which, with the exception of about a quarter of an inch, was contained in the abscess. In the other case an abscess had formed about the head of the cœcum, which, bursting, had produced general peritoneal inflammation, from some of the contents escaping into the peritoneum. In this case there was a tumour during life, in the region of the cœcum. Such cases were by no means unfrequent.

Dr. BURNE had, since the paper which had been read was sent to the Society, seen a case in which the appendix vermiformis was situated in the pelvis, the first case of the kind he had ever seen. In this instance there was abdominal inflammation with obstruction. Pain was referred to the region of the navel, and underneath this was a hardness; these symptoms were relieved, but occurred again, and the patient perished. It was supposed here that the peritoneum was perforated. Ulceration of the ileum was detected at the exact point to which the pain, during life, had been referred. Nothing was observable in the colon; the appendix was found disorganised and hanging into the pelvis, all the contents of which were inflamed. The situation of the appendix vermiformis varied so much in different individuals, that the pain, when this organ was diseased, would, of course, be so differently located, that it was difficult, sometimes, to conjecture that the cœcum was diseased. Cases of cœcal disease were very common; they would be found much more constantly than they were at present, if attention was more particularly directed to them.

Mr. MACILWAIN said that Dr. Burne's paper contained a great many observations, and a great deal of reasoning, which were, to say the least of them, exceedingly open to discussion. At that late hour he should not enter fully into the subject, but merely ask

Dr. Burne if the ordinary treatment of inflammation of the cœcum consisted in the administration of calomel and opium with saline purgatives?

Dr. Burne had usually adopted this treatment, with the best success. Saline purgatives he had found more effectual than those of a more drastic quality, and more successful in relieving that feeling of desire which patients with cœcal disease suffered from, of having more and more evacuations. Calomel and opium were exceedingly useful in the disease.

Mr. Macilwain had asked the question respecting saline purgatives because his experience respecting their use in cases of strangulated hernia, had led him to believe that they were very often productive of inflammation. Cases in which irritation existed in the cœcum, might, by the use of saline purgatives, be converted into cases of inflammation. As the paper might admit of much discussion, he would merely throw out a hint as to the propriety of adjourning the debate till the next meeting.

Dr. P. N. Kingston said it would be curious to inquire into the state of the intestine in those cases of inflammation and ulceration of the cœcum and its appendix which recovered. He had met with cases bearing upon the point. A young woman affected with continued fever recovered, and was attacked with inflammation of the membranes of the brain. Two months after convalescence from this disease she died from an attack of erysipelas of the face. In this case there was complete perforation of the sigmoid flexure of the colon. Adhesion had taken place and obliterated the perforation, which had been one-third of an inch in circumference. There was no symptom of existing disease. In the second case the patient died from disease of the chest. On examining the outer surface of the jejunum he observed a small, perfectly transparent spot, and on cutting into it, found it was the peritoneal tunic of the intestine, the mucous and muscular tissues being entirely gone. The peritoneal coat was perfectly transparent and natural, and there was no trace of inflammation around. In these two cases it would appear that there had been a healing of ulcerative perforation in the jejunum and sigmoid flexure.

It was now a quarter past ten, and Dr. Bright rose to adjourn the Society, when

Dr. Watson said, it was to be regretted that the discussion should be so brief as at the present meeting. The chief object of the Society was the discussion of papers read before the members. The clashing of opinions was useful. The patience of members was exhausted by the reading of long papers on which there was no discussion.

Mr. Arnott agreed with the last speaker, that the most important object of the Society was a discussion of the papers brought forward. It had been usual, in cases of long papers, which there was not time to discuss, for the President to keep the chair until eleven o'clock. The adjournment of the discussion to a future evening would make the Society a mere arena for debate, which it was desirable to avoid. Members did not come to hear papers simply read, they could peruse them, when printed, at home. They came to the Society for the purpose of hearing the conversation which took place.

Dr. Bright would willingly sit in the chair until eleven o'clock, if a motion was made and carried to that effect. He had seen one or two members depart, and had risen to adjourn the Society.

No motion being made the Society adjourned.

*** Dr. Burne's paper occupied one hour in reading, leaving only twenty minutes for discussion.—Rep. L.

WESTMINSTER MEDICAL SOCIETY.

Saturday, December 1, 1838.

DELIRIUM TREMENS—THE MAN FOUND DEAD IN ST. MICHAEL'S CHURCH.

A conversation took place on the often-discussed subject of delirium tremens, in which nothing novel or striking was elicited. It was generally agreed that the affection improperly termed delirium tremens, differed in respect to its cause and treatment; it might be the result of the abstraction of spirituous liquors, or arise from the constant irritation produced by their use; or it might come on in persons of the most temperate habits from great mental affliction. An exquisite case of delirium tremens was related in which the exciting cause was "love and not liquor." The pathology differed; marks of inflammation might exist, or the brain might be apparently in "perfect health." The treatment must be regulated according to the indications presented.

Dr. A. T. Thomson considered that delirium tremens required a peculiar habit of body in the affected person, similar, perhaps, to the hysterical diathesis in women.

DEATH FROM CARBONIC ACID.—QUANTITY OF POISON GIVEN OUT FROM CHARCOAL STOVES.—WHAT IS THE MINIMUM QUANTITY OF CARBONIC ACID IN THE ATMOSPHERE WHICH WILL DESTROY LIFE?—REMARKABLE CASES OF POISONING BY THIS AGENT.

Mr. Snow asked Dr. G. Bird if, in the statement he had made to the Society at the last meeting, that from eight to ten per cent. of carbonic acid gas in the atmosphere would be fatal to life, he meant that quantity of gas mixed with the atmosphere in experiment, or arising from a brewer's vat, or

when produced by respiration, or by the burning of a stove?

Dr. BIRD replied, that as few persons had had experience with respect to cases of poisoning by carbonic acid, we were guided in the expression of our opinions by the experiments of others, or those performed by ourselves, and these experiments had led to the conclusion that an atmosphere impregnated with from eight to ten per cent. of carbonic acid, would be destructive to life. He did not wish, at the present moment, to enter on the subject, as he was pursuing a series of experiments which he trusted would throw some light on it.

Mr. SNOW had asked the question because neither Christison, Orfila, nor any of the numerous authors to whom he had referred, made a distinction between the two cases. He was prepared to show there was a very wide difference. Death, he believed, in neither case arose from any sedative effect of carbonic acid gas, but from the diminished quantity of oxygen in a given volume of air; the lungs, as was known, were capable of extracting only a certain portion of the oxygen from another gas, owing to the affinity which one gas bore to another. Now, when charcoal was burnt, or the gas was given out by respiration, the gas was formed at the expence of the oxygen of the air, and hence it must follow that an atmosphere containing a given quantity of carbonic acid gas arising from these causes, would be equally deleterious with one in which there were five times the quantity introduced by simple mixture. Tables were now shown to prove that about one and two-thirds per cent. of carbonic acid gas, produced by the burning of charcoal, formed an atmosphere in which the oxygen was as much diminished as though eight per cent. of the carbonic acid gas had been introduced into it by simple mixture; it would hence follow that, for the production of an atmosphere equal, as regarded oxygen, to one in which eight or ten per cent. of carbonic acid gas was formed from the burning of a stove, the large quantities of more than thirty-seven and forty-eight per cent. must be added by simple mixture. The reasons why carbonic acid gas was supposed to have a sedative effect were chiefly these:—First. Collard De Martiny had found that, when he substituted carbonic acid gas for the nitrogen of the atmosphere, animals perished in the short space of two minutes in a mixture containing one volume of oxygen and four volumes of carbonic acid. Death in this case would, however, arise from the mere pungent effect of the large quantity of the carbonic acid on the glottis, producing its closure, and, consequently, asphyxia. Secondly. That when a solution of the gas was taken into the stomach, as in soda-water, its absorption sometimes produced sedative effects; there was no proof, however, that the gas could be absorbed by the lungs, and the fact that we were giving it out from them every minute of our lives, offered a strong presumption to the contrary. Thirdly. That a person having his body inclosed in carbonic acid, and breathing the natural atmosphere, suffered from disagreeable sedative effects. Now, the gas might be absorbed by the skin, and not by the lungs. Dr. Edwards, of Paris, had, indeed, fully proved that a certain extent of respiration was performed by the skin, and the mere suspension of this function would produce the effects alluded to. Judging from his (Mr. S.'s) own feelings, when respiring an artificial atmosphere, it made no difference whether a portion of carbonic acid was present or not, provided the quantity of oxygen was the same. He had also found that the presence or absence of carbonic acid exerted little or no influence over the combustion of a common candle, when the oxygen remained the same. On comparing his own experiments on combustion, with those of Dr. Edwards on animals, he had found that animals would live in an atmosphere containing much less oxygen than was required to support a burning candle; yet Dr. Christison relates the case of a girl who fell insensible in a beer cellar, while a candle continued to burn. This fact illustrated the difference which constitution exercises over the effects of poisons. Dr. Reid, at the late meeting of the British Association, had stated that ten per cent. of carbonic acid gas caused uneasy feelings in the dark, though it could be well supported in a strong light. Two per cent., as he (Mr. S.) had shown, produced from a burning stove, would equal the ten per cent. by mixture. Dr. Christison had shown that persons would die of apoplexy in an atmosphere which produced no disagreeable feelings at first; an atmosphere, then, in which charcoal has been burning, containing but a very small quantity of the gas, would be sufficient to cause death; he (Mr. Snow) had that evening inhaled a mixture of ten, twenty, and forty per cent. of the gas with common air, and it had not even produced irritation of the fauces. He considered the statement in Dr. Thomson's "Materia Medica" would be found correct, that when carbonic acid was pure it produced closure of the glottis, and that this effect could be produced when the gas was mixed with an equal bulk of common air.

Dr. GOLDING BIRD considered the physiological effects of carbonic acid gas would be the same when the quantity was similar, whether the gas was added to the atmosphere, or the atmosphere was reduced of its oxygen by the combustion of charcoal. One important deduction was to be obtained from the observations of Mr. Snow: if, for instance, in a small chamber, containing 110,000 cubic feet of atmosphere, 49 pounds

of charcoal were to be burnt in 11 or 12 hours, 1500 cubic feet of oxygen would be absorbed, and not only this, but 9000 cubic feet of irrespirable nitrogen would be set free.

Dr. A. T. Thomson said, it would be interesting to determine the difference between the sedative and suffocative influence of carbonic acid gas. In cases of suicide by this agent, from the party lying near a stove in which charcoal was burning, the gas was taken into the lungs, and death produced gradually from apoplexy. When the person was suddenly immersed in an atmosphere containing the gas the air-passages closed, and the person died of suffocation. Dr. Priestly had proved the sedative power of carbonic acid, for, having burnt his hand, he placed it in a jar of this gas, and the pain was instantly relieved.

Mr. Snow said, the relief obtained in this case arose, probably, from the removal of the hand from the irritating influence of the oxygen of the air, and not from any sedative effect of the gas itself.

Mr. Everitt thought it would be most interesting to ascertain, by experiment, how much less than ten per cent. of carbonic acid gas in the atmosphere could be breathed for a long time without danger. Saussure had made many thousands of experiments on the atmosphere, taken from high and low situations, and at all seasons, and the mean maximum of carbonic acid contained in it was 4 in 10,000. Now, it would be a point of much importance to determine how far this quantity could be increased with impunity; it was difficult to arrive at this conclusion. It was said, on the late inquest, that the atmosphere in the church contained one per cent. of carbonic acid only; yet what an increase was this on the maximum contained in the natural atmosphere! Some people were particularly sensitive with regard to this gas. He had found, when lecturing at public institutions, that the gas evolved from a very small charcoal chafer so affected two or three ladies that they were obliged to leave the room; others had observed this fact as well as himself, and gentlemen who sat near him during his lecture had requested him not to burn charcoal, as it produced headach and other unpleasant symptoms. He was inclined to believe that carbonic acid, obtained from the burning of charcoal, was more deleterious than that produced from chalk, or by fermentation. Now, there was a wide range between ten per cent. and the maximum quantity found in the atmosphere. How far beyond this latter could we safely go? With regard to himself, he should be unwilling to increase much the quantity which Nature had given; he should be unwilling to breathe an atmosphere containing one per cent. of the gas. In considering this question he thought we ought to sail far on the safe side.

Dr. G. Bird agreed with Mr. Everitt that some individuals were much more readily affected by the gas than others. Notes of three cases illustrating this he had in his possession. The first of these occurred in Paris: two sisters slept together in a room in which there was charcoal burning in a chafing-dish; one sister was alive the following morning, the other dead. In the second case, a young woman, previously in good health, with a child, aged two months, and her husband, slept in a room in which there was a good-sized chimney; a chafing-dish, containing burning charcoal, was placed near the chimney; the husband, on awaking in the morning, found his wife dead, and the child in a state of asphyxia. The third case was that of an assistant apothecary, who filled a warming-pan with charcoal in a small room in which he was at work; in a few minutes he became asphyxiated; a person in the room, perhaps a little farther off from the warming-pan, suffered nothing.

In answer to a question,

Mr. Everitt said, it was a chemical law that all gases diffused themselves uniformly if time were allowed for this to take place; carbonic acid, for instance, evolving from a brewer's vat, would, at first, fall to the ground, but would soon become mixed with the atmosphere of the room; the carbonic acid gas given off from a burning stove, being of a higher temperature than the surrounding air, would ascend, but the agitation of the air in the room would soon mix it equally around; there would be no more danger close to the stove than away from it, with the exception that the heat might be injurious. In all stoves in which there was not a free ventilation the air of the apartment acquired a greater capacity for moisture, and, unless some artificial means were resorted to, to correct this, breathing would become oppressed.

Mr. Chance referred to some valuable experiments on burning charcoal, by Mr. Coathupe, which were inserted in a late Number of The Lancet.*

Dr. Bird said that the objection against stoves not having a free ventilation held good with respect even to Dr. Arnott's invention; to prevent the excessive dryness of the air arising from its use, he (Dr. B.) had adopted a very simple plan, with complete effect; he had a small flat dish, with a perforated cover, placed on the top of the stove; in this dish he placed a piece of moistened sponge, the evaporation going on from which rendered the air moist and pleasant. He agreed with Mr. Everitt with respect to the admixture of gases in a small room quickly taking place, and that, at first, the specifically lighter one, as in the case of the heated carbonic acid, would

* See Lancet, No. 8, p. 262.

ascend; in a larger building, such as St. Michael's Church, however, as the heated gas had a great number of feet to rise, he had found, as far as experiments had gone, that the gas ascended only a few feet, and then, giving out its caloric, fell downwards. From one opening in the large stove in the above church the carbonic acid might be caught in a vessel as it was descending; of course, however, time, even in this building, would produce an equable mixture of the gases.

PROGRESS
OF THE
BRITISH MEDICAL ASSOCIATION.

THE following circular has been addressed to the medical gentlemen of Nottinghamshire and the adjacent counties, and it is leading to the adoption of measures for forming similar influential and important branches of the British Association in the chief county towns of the circle embraced in the distribution of the address:—

SIR:—A Branch of the BRITISH MEDICAL ASSOCIATION having been established for Nottingham, Nottinghamshire, and the adjoining counties, at a meeting of medical practitioners of this town and neighbourhood, held here September 26th, the Committee direct me, respectfully, to solicit you to become a member thereof.

The present urgent necessity of defending and upholding the rights, privileges, and respectability of our profession—of repressing illegal practitioners—the advantages of establishing a benevolent fund—of promoting kind and honourable feelings towards each other—together with other important matters, are too obvious to be more than alluded to; and such great results are only to be attained by the moral power of united exertion and unanimous perseverance.

Illustrative of the practicability of effecting such extensive objects, allow me to remind you that the BRITISH MEDICAL ASSOCIATION has already succeeded in procuring every modification, excepting one, of the New Poor-Law system, which that Association suggested to Government,—a degree of success which should urge us to continued efforts.

It is proper to inform you, that of the annual subscription (which is a guinea), one-third is devoted to the Benevolent Fund, for decayed members and their families. The rules of the Society (to be had of the Secretary) will further explain the nature of the Association. I have the honour to be, Sir, your obedient servant,

HENRY C. ATTENBURROW,
Nottingham. Honorary Secretary.

OFFICERS FOR 1838-9.

President.—Dr. J. M. Davidson, Senior Physician of the General Hospital, Nottingham.

Vice-Presidents.

Mr. Wm. Wright, Surgeon to the General Hospital, Nottingham.

Dr. Howitt, Physician to the General Hospital and Dispensary, Nottingham.

Mr. Cooper, Surgeon, Mansfield.

Dr. Hutchinson, Physician to the General Hospital, Nottingham.

Mr. Greeves, Surgeon to the Dispensary, Nottingham.

Treasurer.—Mr. R. Davison, Senior Surgeon to the Dispensary.

Honorary Secretary.—Mr. H. C. Attenburrow, Surgeon to the County Gaol.

Committee.

Mr. G. M. White, Surgeon to the General Hospital.

Mr. Massey, Surgeon to the Dispensary, and Consulting Surgeon to the Union Hospital.

Mr. Billings; Mr. Northon Thompson; Mr. Orton, Beeston; Mr. Burrows; Mr. Valentine, Sutton-in-Ashfield.

"EIGHT POUNDS A YEAR!"
NOTE FROM MR. TUCKER.

To the Editor of THE LANCET.

SIR:—Had I been the author of the advertisement which "MEDICUS" thought wise to lay before your readers, in the last Number of THE LANCET, I should have felt degraded at beholding it in your Journal. My "blush" would have been a blush of shame, not of indignation.

I was not the author, nor knew I aught of it until my attention was drawn to ✱ on the day of its appearance. "MEDICUS" would lead your readers to believe that I am in the habit of issuing advertisements of a similar nature. I declare, I never, in the whole course of my practice, sent even one to a medical journal or a newspaper, nor did I ever authorise any one to do so for me. Those who know me, and my sentiments with regard to medical assistants, will believe my assertion; to those who know me not, I beg to say I possess proofs that I was not the originator of the one in question. My motives for withholding the name of the real advertiser may be divined, they never will be explained. Suffice it to say, that no medical man had any hand in the affair. I am, Sir, your obedient servant,

J. H. TUCKER.

38, Berners-street, Dec. 4, 1838.

*** Mr. Tucker, we are sure, is too respectable a practitioner to offer an assistant eight pounds a year, but we are bound to tell him that the above letter is to us perfectly incomprehensible.

DOSES OF ACETATE OF MORPHIA.—*To the Editor.*—SIR:—In reply to your query addressed to the profession in the last Number of THE LANCET, I beg to state that I have administered one grain of the acetate of morphia in the form of pill, and in combination with the confection of roses, to patients afflicted with chronic rheumatism; and, in a case of paralysis attended with severe pain, occurring in an individual of a rheumatic diathesis, I directed one grain, in the same form, to be given at bed-time, whenever the pain supervened, with the most satisfactory results. I am, Sir, your faithful servant,

G. P. HEYWARD.

Nov. 30, 1838.

EFFECTS OF

DIFFICIENCY OF CERUMEN

ON THE

FUNCTION OF HEARING.

To the Editor of THE LANCET.

SIR:—The translation of Dr. Kramer's work on "Diseases of the Ear," a work which is, undoubtedly, the most scientific that has ever appeared upon this subject, and its author one whose opinion upon diseases of the ear is, beyond all doubt, more valuable than that of any one living, contains, at page 94, the following passage:—"Deviations in the quantity and quality of the cerumen are generally unimportant attendants on other diseases of the ear, but very seldom exist independently, *and then exert no important influence on the function of the organ.*"

Experience enabling me entirely to concur with the truth of the principle contained in the paragraph just quoted, I was surprised to find in THE LANCET, of November 23rd, a long letter from Mr. Curtis, extracts from which have been liberally circulated by means of the daily papers, in which letter he states, that *one of the principal, and most common causes of deafness, is a deficiency of the secretion from a want of action in the ceruminous glands.* To restore the function of the ear, by causing a proper secretion of the cerumen, Mr. Curtis lays claim to the discovery of the advantage of a local application of creosote. Five cases are related by Mr. Curtis, as proofs of the value of his discovery. The subject of the first case was a man, ætat. 53, "who had been deaf for ten years; was so deaf that he could not hear the ticking of a watch, even when placed close to the side of the head." In this case, the membrana and cavitas tympani being healthy, the Eustachian tube pervious, and the internal ear in a normal condition, will any one believe that the vibrations of air, arising from the ticking of a watch, when "placed close to the side of the head," would be prevented from falling upon the membrana tympani *because the meatus was not lined by cerumen?* I think that no one would believe this, and yet Mr. Curtis is the voucher for the truth of it. In the second case, the patient had been deaf for five years, and "creosote gave relief in a fortnight."

The subject of the third case "had been deaf for six years, *owing to irregular and disorderly action of the chylopoietic viscera*, nevertheless here, also, "creosote was used to advantage."

In the fourth case, a lady "had a confirmed deafness of thirteen years duration;" creosote here, also, was the remedial agent, and the "cure* was effected in about a month."

The subject of the fifth case had a "*deafness from infancy*," still the creosote relieved her.

For Mr. Curtis's sake I refrain from making any further remarks upon these cases, and will, in conclusion, add, that, in the room with me, while I write, there is a medical gentleman who had no secretion of cerumen for some years, during which time *the function of the organ was not in the least degree impaired;* and I have under my care at the present time a patient affected with scrofulous inflammation of the soft parietes of the meatus, the constriction of which is so great as scarcely to admit of the passage of a crow-quill. When this patient first consulted me there was also a very abundant discharge from the meatus, and yet the vibrations produced by the ticking of a watch, were heard at a distance of two feet six inches from the external ear, and the patient could carry on a conversation without suspicion of there being any disease of the ear.

To set this question at rest, let Mr. Curtis come forward and authenticate his cases, and I will instantly do the same with mine; and should it, by this means, be proved that Mr. C. is entitled to the credit and honour of the valuable discovery, which the paragraphs of the daily papers attach to him, then, Sir, no one will be more happy to join with the community in rendering to Mr. Curtis his desert than your obedient servant,

J. T.

MEDICAL JURISPRUDENCE.

BY what signs are we enabled to determine whether a man has been hanged before or after death? This is an important question in medical jurisprudence which has not yet received a decisive answer. Had such been the case, the death of the Prince de Condé would not have been involved in an

* We are left to suppose that the deafnes in these cases was caused by a deficiency of cerumen.

obscurity which can never be dissipated. M. Devergie has lately determined one or two circumstances likely to throw some light on the question. When a man has been hanged during his life, a quantity of semen is always found either about the orifice or in the cavity of the urethra. Another sign, more positive still, is the congested condition of the corpora cavernosa, which contain more or less extravasated blood in their cells. The above phenomena are never found in the bodies of men who have been hung up after the extinction of life from other causes.—*French Lancet*, Nov. 22, 1837.

ON THE

USE OF CAMPHOR

IN CERTAIN

AFFECTIONS OF THE RESPIRATORY APPARATUS.

By F. V. RASPAIL.

THE following is the substance of a letter which M. Raspail has recently addressed to several of the French medical journals. It is seldom that we pay any attention to the proposal of remedial agents by non-medical men, but the distinguished character of M. Raspail, both as a chemist and an observer of Nature, entitle any remarks which fall from his pen to more than ordinary attention :—

The substance which M. Raspail recommends to the notice of medical practitioners is camphor. It may be used in two forms: a piece of camphor is placed in a small tube of straw, or in a small quill, and this is formed into a little cigar, which the patient may smoke, not in a state of ignition, but cold, by simply inhaling the air through it. The saliva excited by inspiring the camphor should be swallowed. The second form consists in the application of a piece of lint, moistened with a saturated alcoholic solution of camphor, and covered with a piece of oiled silk, caoutchouc, or any other impermeable substance, to the affected part.

M. Raspail assures us, as the result of considerable experience, that in all cases of respiratory affections, such as those popularly denominated cold, catarrh, influenza, &c., the constant use of the camphor cigar and lotion will produce speedy amendment, and when the lungs are merely congested, almost instantaneous relief. He has also seen some cases which lead him to believe that the constant use of camphor, in the way just mentioned, is capable of dissipating the incipient symptoms of pulmonary consumption. The pain occasioned by adherence of the two pleuræ, popularly known by the term "stitch in the side," M. Raspail has seen dissipated in a wonderfully short space of time, by the application of the camphorated compress, and the use of the cigar. There are several other affections and diseases in which M. Raspail thinks that camphor might be employed with great advantage; but we think it sufficient, for the present, to direct attention more particularly to those of the respiratory apparatus. At the conclusion of his letter the author assures us that his communication has been made solely from a desire of benefitting his fellow men, and with a hope that medical practitioners will repeat his experiments on an extensive scale, the more especially as the remedy, unlike so many others, can do no harm, if it effect no good.—*French Lancet*, Nov. 17, 1838.

DUTCH AUCTIONS IN THE LIBERAL PROFESSIONS.

To the Editor of THE LANCET.

SIR :—In the excellent lecture of Professor Maunsell on medical reform, in THE LANCET of this week, some very pertinent observations are introduced on the infamous system of setting up the medical attendance of the sick-poor to *Dutch auction*. It appears to me, however, that this infamous practice has not yet been submitted to the political authorities in a manner which is on a level with their comprehension. I beg, therefore, to give a hint on the subject, which might be useful to those who may be engaged in exposing this deplorable practice.

The question always asked by the grinders of the poor is this:—"Why not take the cheapest tender, if the person who tenders possesses the legal qualification?" You have only to grant this of one profession, and it applies with equal justice to all others. If you want a bishop for Durham, why not sell the office by Dutch auction? I aver that plenty of well-educated clergymen would emerge from college, and do the duty for 200*l*. a year. But we should soon see the consequences to the Church and the public. Just so of the Army and Navy; and of the Law too; for if you were to put up the offices of Queen's Counsel, of Masters in Chancery, of Judges, &c. &c., why, there are hundreds of young barristers, properly educated and legally qualified, who would be glad to take any of the offices for 100*l*., if it were only for some sort of beginning. Besides, the situation would afford them such admirable advantages in the way of practice and experience! Let any sensible man say, however, whether both the public and the law would not suffer, as much as the poor public and the medical practitioner now suffer from this cruel absurdity. I am, Sir, your obedient, humble servant,

PHILIATRAS.

Nov. 24th, 1838.

A "CORRESPONDENT" of "The London Medical Gazette" has endeavoured to call attention to what he calls "blowing hot and cold on Dr. JACOB," in two notices which have lately appeared in the columns of THE LANCET,—the first of them having been a note which was appended to the letter of Dr. MAUNSELL, and the second an editorial notice, which was published at page 351 of No. 795. If the "Correspondent" of the "Medical Gazette," had printed the notices as he found them, and, at the same time, had understood their meaning, he would not have applied the terms "hot and cold" to two articles which were of precisely the same temperature. It was the "Correspondent" who caused the word "precious" to be printed in *italics*, and not THE LANCET. But, by putting it in those distinguishing characters, he has, of course, given to the whole paragraph the appearance of having been written in irony. We said at first, but in other words, that the address of Professor JACOB on medical reform was a valuable one. We said the same thing a second time; and now we wish to be understood as saying it again.

THE strange sayings and doings at the WEBB-STREET SCHOOL, deserve a notice in this Journal, and they shall receive it. Alas! Is the name of "GRAINGER" to be classed amongst those of the anti-medical reformers? Ghost of EDWARD GRAINGER, arise, and dispel the impending gloom! How the corruptionists and toad-eaters must laugh at the apparent pusillanimity of the present head of the "opposition" school! It is well that *principles* do not change with *men*, and that the cause of Truth ever stands unshaken amidst the surf and surge of those interested feelings and passions which so often agitate the human breast. One of the spirited reformers of the school should read the HISTORY of that establishment to the Medical Society, with the present Mr. GRAINGER sitting as President. The act would be both noble and courageous, and might prove almost a divine sacrifice at the altar of Justice. The *materiel* for the history is to be found in the columns of THE LANCET. If any inquiries be made on the subject we shall be more particular in pointing them out. We feel a great interest in the reputation o the present Mr. GRAINGER, because THE LANCET prevented the *now* lauded College of Surgeons from destroying him, as a lecturer, on the death of his lamented brother, in January, 1824. In the meantime, the students should respectfully solicit that Mr. RICHARD GRAINGER would have the kindness to publish the correspondence which passed between the Council of the College of Surgeons and himself in the spring of the same year, when that now bepraised Council made a desperate effort to *put down* the Webb-street School, and (as one of that worthy body said), "prevent any "two bricks of the detestable establishment "from standing one upon the other." But more of this anon. The whole history must out, and once again blacken our pages.

TO CORRESPONDENTS.

THE communications relative to the doses and effects of MORPHIA have been so numerous that we could not publish a twentieth part of them. The contents of the paper which was forwarded to us by *Mr. Pemberton*, exhibited some interesting facts, but no novelties. Some extracts may be given from a few of the letters on a future occasion. We embrace this opportunity to thank our correspondents for their prompt replies to our question.

Communications have been received from *Mr. Mirehouse*, *Dr. Congreve Selwyn*, *Mr. George Rees*, *Mr. George Lucas*, *Dr. Flood*, *Mr. James Freeman*, and *Mr. Ancell*.

We cannot discuss the question submitted to us by *X. Y. Z.*

The newspaper from the Isle of Jersey has been received.

We have already stated, in a notice to correspondents, that we cannot insert any communications in support of the exploded humbug of animal magnetism.

Dr. Selwyn's paper on Encysted Dropsy of the Thyroid Gland, with a method of operation and cure, shall appear in the next LANCET.

The subject of *Dr. Nicol's* note is under consideration.—We have received the letters of *Mr. George*, *Mr. G. Ross*, and *R.P.*

The letter of *Mr. C. Chapman, jun.*, next week. It would be more useful to publish their names.

ERRATA.—In the letter of Mr. Heyward, Nov. 21, page 331, line 10 from bottom, *for* "½ ounce," *read* 5½ ounces. Page 332, line 3, *for* "tinct.," *read* wine; line 8, *for* "½ ounce," *read* 5½ ounces. The reader is requested to take a pen and make these corrections at once.